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Vol. 237 MAY, 1944 No. 5

METEOROLOGY IN THE FRANKLIN INSTITUTE.

BY

ARMAND N. SPITZ.

(PART II.)

After Professor Espy (Fig. 5) left The Franklin Institute to become the first Meteorologist of the United States government, there was a period during which the Institute's activity in this field was, although not entirely dormant, by no means as active as it had been. Reports continued to come in from all over the country, and they were correlated with the work of Espy in Washington, but, for more than a decade, the Institute's leadership in the field flagged markedly.

The observers who had been making weather records continued them, in some cases, for many years. New observers were found, and were supplied with blanks for making their "journals."

In 1854 the Committee on Meteorology issued a new edition of "Directions for Making Meteorological Observations" and distributed it widely to its correspondent observers in the United States and other countries.

The Smithsonian Institution's program of studies was of growing interest. An attempt was being made to set up a weather reporting service far more extended than anything that had yet been suggested. Under Joseph Henry the system was gradually developed, and by 1854 telegraph offices were furnishing the Institution with weather data every day, from which, for several years, synoptic maps were drawn and displayed.

The Meteorological Committee of The Franklin Institute continued to function, but its activities were more a matter of routine than of productive research. At the suggestion of Professor John Wise in 1870 it was decided to form a Meteorological Section which would devote

(Note—The Franklin Institute is not responsible for the statements and opinions advanced by contributors in the JOURNAL.)

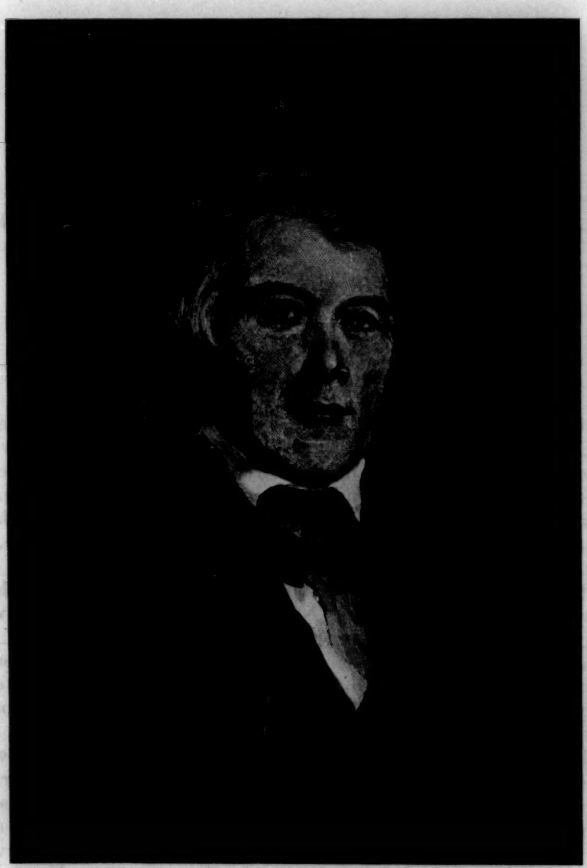

FIG. 5. James Pollard Espy, who began his investigations in meteorology as a member of The Franklin Institute's faculty. He later became chairman of the Joint Committee on Meteorology of the Institute and the American Philosophical Society, and then was named as the Institute's Meteorologist. In 1842 he was appointed by Congress to be Meteorologist to the United States Government, serving under the Surgeon-General. (Woodcut from *Popular Science Monthly*, April 1889.)

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itself more actively to the study of meteorology and its development. The suggestion was heartily approved.

The organization meeting of the new Meteorological Section was held in the Hall of the Institute on Christmas Eve, 1870. Fairman Rogers was nominated as president, Hector Orr, John Wise and Henry Morton as vice presidents, W. H. Wahl, secretary of the Institute, was elected recorder, Thomas S. Speakman as treasurer, Robert E. Rodgers as Corresponding Secretary, and Basil Seward and John Wise were named conservators. After appointing a committee to draw up regulations, the chairman addressed the meeting outlining the platform, as it were, of the new group, foreseeing many of the twentieth century applications of Meteorology:

"Gentlemen: It has been averred that in general no American neglects any fair opportunity of making a speech. I do not think that I am excessively addicted to this weakness, but I would like to say a few words before we separate.

"We have now organized the 'Meteorological Section' of the Franklin Institute for the prosecution of a much neglected branch of science. . . . The importance of our main object in this association is undeniable. Meteorology will be found to cover a wide domain of experiment: extending to the ascertainment of the composition, density and temperature of the air from the lowest levels to the greatest heights; and involving questions of health and disease. It embraces the formation of clouds, the cause and career of storms, with their majestic phenomena of lightning and tornado, and these in turn control or modify climate, which determines the amount and kind of natural production; and these affect both Manufacture and Commerce. And further—the investigation of air navigation is before us, by which a truly royal road of intercourse may yet be found for the inhabitants of opposite hemispheres.

"I look upon our original list of names with solid gratification: to me it seems full of promise. I find myself the only nobody of the company, and I must endeavor to make up in zeal what I lack in knowledge. I thank you for the devotion to Science which you have shown by thus assembling in this inclement weather, and on this particular evening, so full of genuine attractiveness in the homes of our city; but in return let me assure you that I think you will find the experiment worth its cost, and that upon this Christmas Eve of December, 1870, we have set our hands to a work which (however long we may live) we shall never be ashamed of, nor will those who may worthily come after us." [30]

Professor Wise then rose and said: ". . . I most heartily second all that you have said about the importance of the task now before us. I

[30] Minutes of the Meteorological Section of The Franklin Institute, p. 3. This volume is on display in the Hall of Weather.

have given it some attention through my whole life, and for the past forty years I have followed it closely. The only truly scientific authority which has given constant and systematic attention to Meteorology on this continent (I believe) is our Smithsonian Institute at Washington City, and I have frequently conferred on this subject with Professor Henry, its able and indefatigable head. I therefore think that we have begun a work here tonight in which we shall earn the thanks of our countrymen far and near.

"I feel much encouraged by the promptness with which the Franklin Institute has acted upon my suggestions, and I pledge my best personal exertions towards bringing successful results to our enterprise. The consideration of the means of protection to life and property from the effects of lightning would of itself be an object worthy of our best efforts; and when we extend our views so as to embrace sanitary effects, and also to the establishment of navigation of the air, we shall find that our hands are full of honorable labor which cannot fail of its reward."

At subsequent meetings a wide variety of meteorological subjects was discussed, including the connection between ozone and atmospheric electricity and various types of thunderstorms.

In May, 1871 the president was instructed "to solicit of the various Rail Road Companies, whose routes radiate from Philadelphia, the granting of a free pass over their respective roads: said pass to be used by a person, designated by the Section for the purpose of instituting a personal and Scientific investigation of any cases of accident or disaster, caused by electricity, or other Atmospheric Agencies; in order that, by careful collection and proper interpretation of many facts, proper means may be acquired for the more perfect protection of life and property: thus contributing to the advancement of Science and the general welfare." [31]

On September 4, 1871 the Section appointed a committee to "put in order the original Electrical Machine of Dr. Franklin, at the expense of the Section." [32]

The fame of the Institute's activities in meteorology spread far and wide. On October 2, 1871 it was reported that the mayor of Iowa City had communicated with the Section with regard to the effects of lightning upon street mains during a recent storm.

In February, 1872 it was decided to investigate the disposition which had been made of the instruments which had been distributed a third of a century before. No further record has so far been located of the findings of this committee. It seems too much to hope that now, three-quarters of a century after the investigating committee was appointed, any light may still be thrown upon this subject, but the instruments

[31] Minutes of the Meteorological Section of The Franklin Institute, p. 9.

[32] This machine, now on display in the Hall of Electrical Communications, is one of the Institute's most highly prized possessions.

were distributed, and may very well still be in existence, in some out-of-the-way places. Any information will be eagerly welcomed.

At about this time there grew a fairly wide popular interest in the studies of the weather being conducted by the Institute, and local newspapers carried full reports of papers presented on such subjects as "Clouds and Their Functions," "What Floats in the Air," "Lightning Rods," "Spontaneous Combustion," and various studies concerning the relation between the atmosphere and health.

On July 1, 1872 Professor Wise volunteered to postpone the reading of his paper on "The Geology of the Atmosphere," "on account of the extreme heat and the faint light afforded by the gas."

Dr. J. J. Woodward, assistant surgeon of the United States Army, reported in December concerning an apparatus used by him to collect atmospheric particles. A contribution of one hundred dollars was made for the purpose of studying the nature and composition of such particles.

Among the correspondents who worked with the Institute's meteorological group were such names as J. Norman Lockyer, of London, Father Secchi, of Rome, Cleveland Abbe and Joseph Henry, of Washington, and many others in many lands.

One of the first suggestions anywhere to be advanced that there was tremendous meteorological value in upper air observation was presented in a paper, "On the Balloon as an Instrument in Meteorological Research," by Professor Wise.[33] The proposal had been made at a special meeting of the Section.

"In the study of meteorology there is one means of investigation which is capable of marvelous development, and which, as yet, has not received a tithe of the attention of which it is worthy. I refer to the balloon. Meteorology has as yet feeble claims to the name of Science, nor, until some mode of research is devised that will bring the student of atmospheric phenomena into closer relation, into more direct contact with the problems he would solve, can we hope for more rapid progress than we make at present. The air-ship must furnish us with the solution of the problems, at which we can now but venture to guess—for it is the only means which is competent to meet the imperative wants of the investigator; it alone can bring him face to face with, and in the very midst of the mysteries of his subject. It is to meteorology what the water-ship is to hydrography, and in their respective departments one can be as little dispensed with as the other. The little that we know of the subject serves as far as it reaches to justify the comparison. The deep sea soundings, so pregnant with interest in their revelations of infusorial life at the bottom of the ocean, have been met with the deep air soundings, revealing to us vegetable myriads which make the upper air their highway.

[33] JOURNAL OF THE FRANKLIN INSTITUTE, Vol. 91, No. 5, May 1871, p. 345.

"Is it not a marvel then that a ripe field for the sickle waited so long, and waited in vain for the harvester? It will be objected that the mode of investigation is a dangerous one; but, with the experience of many years to warrant the assertion, the writer feels safe in stating that this opinion is wholly erroneous. It has its origin partly in the novelty of the means employed, and partly in wilful misrepresentation. If one who is interested in the subject will take the trouble to institute a comparison between the number of recorded air voyages and the accidents incident thereto, a result will be obtained as favorable as the most sanguine admirer could claim for ocean travel.[34] . . . Let me hope that sufficient has been said and accomplished to prove that we can ascend into the air, into the cloud, into the storm, by day or by night, to investigate the phenomena of the atmosphere, without incurring the accusation of being reckless if we but turn our eyes of yesterday, when high officials of state sail from beleaguered cities in the air-ship, and governments use them as mail route agencies to distribute their daily mails.

"To the Franklin Institute should the honor be assigned of establishing a Section of Meteorology, to explore the mysteries of the atmosphere with the aid of the air-ship and suitable apparatus. The cost of construction with modern facilities has ceased to be a draw-back, and the establishment of gas works has greatly lessened the expense and inconvenience of inflation. And with such men as Dr. Wahl, our efficient Secretary, and his co-laborers in science, to undertake the investigations, and with an experienced air navigator to command the ship, a year would suffice to collect a series of facts which might go far toward deciding authoritatively matters which are now obscured in the mists of doubt and uncertainty. The services of the writer—if his experience is deemed of avail—would be most cordially offered.

"We owe such a course of investigation to the age in which we live. The progressive spirit of science demands earnest toil from her disciples, and calls for generous nourishment from institutions of learning, and none are better fitted to meet her claims than the Franklin Institute.* [35]

"Once properly put in motion, it would not be long before those who now scout the idea of crossing the oceans and circumnavigating the globe with balloons would be as solicitous to make their tours of recreation and scientific investigations in them as they now are to doubt its feasibility."

In February, 1873, Professor Wise announced that he was studying

[34] Here follow a number of detailed descriptions of his experiences of adverse conditions in balloon ascensions, none of which resulted in harm.

[35] * The footnote appended, signed by Dr. Wahl, points out that "a meteorological section has for some time been organized in connection with the Institute, and has been very warmly supported. The object which the author so ably advocates may, when the resources of the section may have grown sufficiently to warrant the undertaking, be practically carried into execution."

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the possibility of making an aerial voyage to Europe, and he was invited to discuss the methods of management of balloons at a subsequent meeting of the Institute. The next reference to this project is in September, 1873, when he wrote to the Section, telling about the "Air Ship, now in course of Preparation at New York for the Trans-Atlantic Voyage to Europe, and announcing that the Graphic Managers had declared that its day of departure should be the 10th inst." The following month, however, Professor Wise was at the meeting held in the Institute. There is no reference to the voyage, but he read a paper on "Ways of the Sea and Ways of the Air." On the same evening a discussion of Andre Voey's new system of cloud classification was held. In November, a resolution was adopted:

"Whereas this Section is giving special attention to the subject of Aerial Navigation, therefore:

"Resolved, that its Corresponding Secretary be, and he is hereby authorized to address, directly, by Letter, or through the Press, a request to all Scientific Associations, or other persons throughout the world, who have been in the last few years making acquisitions of knowledge in relation to the subject of Aerial Navigation, or who may do so in the future, that they communicate with this Institute in regard thereto, in order that, by gathering into one place all attained knowledge on the subject, something practical may be effected." [36]

No record has been found of the vicissitudes through which Wise's trans-Atlantic balloon project may have passed, but in June 1874 he asked the Section to appropriate one hundred dollars to place the balloon in condition to make studies of the constitution of the atmosphere with particular respect to floating particles and ozone (Fig. 6). This grant was made, and future minutes contain his reports of successful ascensions. In February, 1875, he read another paper on "Transatlantic Air Craft."

Mr. Sterling P. Fergusson, Research Associate of Blue Hill Observatory of Harvard University, in a paper on "The Early History of Aerology in the United States," [37] credited Wise with being the first to make any ascensions of importance to meteorology. "He made 466 ascensions for purposes of business. Although Wise's instruments cannot be considered accurate, his observations and descriptions of optical phenomena, storms, clouds, etc., are of great interest."

Throughout the decade and a half of activity of the Meteorological Section, there was a regular collection of data from various parts of the country, systematically recorded and, unfortunately, not so systematically preserved after it was abstracted. The forms of recording observations are interesting, because they show the Institute's develop-

[36] Minutes of the Meteorological Section of The Franklin Institute, p. 35.

[37] *Bulletin of the American Meteorological Society*, November 1933, Vol. 14, p. 252.

Estimate of Balloon Project

Materials necessary to put the Herald Balloon in working order as a gas balloon:

New cloth	$83.00
40 Galls Varnish & Turpentine	90.00
Car & appendages	30.00
	$203.00

Network, Guys, anchor, hoop &c I have all on hand ready for use.

Cost of Inflation, Gas, ballast bags, help &c In Burbes Experiment say $80.00

Instruments to collect Atmospheric Particles, Ozone tests, Thermometer Small Aneroid barometer &c. 35.00

$115.00

If the section will appropriate the above necessary for the Burbes experiment, or only a hundred dollars towards it, I will make it under the direction and auspices of the Meteorological Section. As to the expenses of fitting up the balloon, I will do that at my own cost, unless the Section should desire to use the Balloon for more experiments: In that event the expenses for its use would be a nominal cost of wear and tear: All of which is Respectfully submitted

John Wise

Philada June 12th 1874.

FIG. 6. Proposal from Professor John Wise that the Institute underwrite a series of studies of the upper atmosphere in the interest of meteorology. Money was appropriated for this project, which made history as the first excursions into the upper air with these studies specifically in mind.

ment of the record sheets later to be adopted by the governmental weather services (Fig. 7).

On July 3, 1876, the regular meeting of the section was adjourned "on account of its being the eve of the Centennial Anniversary of our National Independence." In December, of the same year, the Section decided to suspend its activities, and so reported to the officials of the Institute. The Committee on Meteorology, however, continued to function regularly.

In September, 1885, W. N. Jennings, who is, today, still a member of the Institute and of its Committee on Science and the Arts, showed a photograph of a lightning discharge. Beginning in 1882 he had begun to make photographic experiments to prove that lightning never occurs in the zigzag form so commonly seen in pictures. He succeeded in proving the wavy line theory of lightning by means of the camera.

On October 16, 1888, Mr. Jennings addressed a letter to the Institute, recalling the meeting of three years before, after which his photograph had been published,[38] together with his letter referring to the wavy line theory. His new communication enclosed a number of other remarkable photographs, taken between 1882 and 1888, all of them showing the wavy character of the discharge. The outstanding specimens were a photograph of a streak of lightning occurring between two clouds, one from zenith to horizon, a horizontal discharge with numerous branches, and a vertical streak . . . "very remarkable, on account of its lower extremity appearing to be in a molten condition."

"The object of this letter is to place myself on record as being the first practically to prove, by means of the camera, the wavy line theory of lightning, and its identity with a frictional machine spark, the results of which were exhibited in the Lecture Hall of the Franklin Institute in September, 1885. The future developments in this line of photographic research are so promising, that it might become of interest some day to trace its origin." [39]

In a paper on the "Movement of Upper Air Currents," [40] H. Allen Hazen outlined methods of study of the direction and velocity of winds at high altitudes. He suggests the use of toy balloons to make such observations. "It is probable that, at an expense of less than ten cents, a balloon can be made to rise to the height of 3,000 feet, and up to 6,000 feet at less than twenty cents. When we consider the enormous advantage to be derived, even from a single well made ascension of this kind, the above sum is paltry enough; dollars have been spent for single observations of far less value." Hazen's conclusions indicated that upper air winds, in addition to their general eastward motion, also have

[38] *Scientific American*, September 5, 1885.

[39] JOURNAL OF THE FRANKLIN INSTITUTE, Vol. CXXVI, No. 6, Dec. 1888, p. 491.

[40] JOURNAL OF THE FRANKLIN INSTITUTE, Vol. CXXVI, No. 1, July 1888, p. 45 ff.

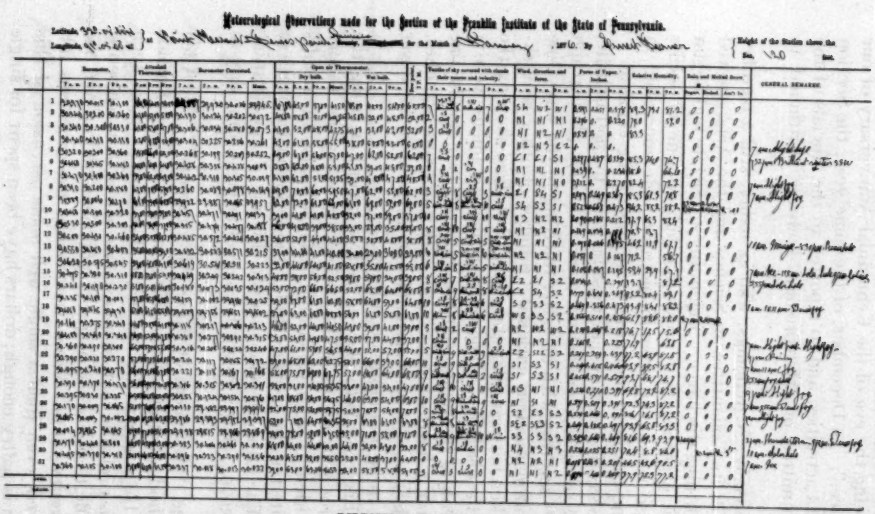
Meteorological Observations made for the Section of the Franklin Institute of the State of Pennsylvania.
DIRECTIONS TO THE OBSERVER.

FIG. 7. The Franklin Institute's form of weather observation record, later adopted by the Pennsylvania State Weather Service and the United States Weather Bureau.

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the typical cyclonic and anticyclonic circulations; that upper cirrus clouds, in general, have a motion similar to that of the currents at about 6,000 feet elevation; that the velocity of currents at about 6,000 feet is less than half at a high area than it is at a low area; that the motion of lower clouds indicates more clearly the direction of the storm than the upper clouds; that the path of an advancing low can be determined from the height and direction of the clouds in front of it; that the intensity of the storm is largely due to the high velocity of the upper currents in the vicinity of a low; that there is a diurnal variation in the velocity of storms independent of the velocity of the accompanying low area due, perhaps, to the rising of storms to an altitude where the currents are of a higher velocity than the center of the low.

On December 17, 1886, Cleveland Abbe delivered a lecture before the Institute, entitled "Popular Errors in Meteorology." He prefaces it, however, with a recital of the eminent scientists who, in these halls, "long since expounded some of the most important laws previously unknown, the knowledge of which has gradually worked a revolution in our views as to the Philosophy of atmospheric processes.

". . . to Espy, an active member of this Society . . . modern meteorology is indebted for such a thorough study of the clouds, of their methods of formation and of the secret of the growth of storms, whether the smallest thunder-storms or the grandest hurricane, as has made him eminently worthy to receive the title 'The Father of Modern Meteorology.' [41]

"Fifty years ago the Franklin Institute appointed a permanent Committee on Meteorology; to that committee we owe the study of meteors by Sears C. Walker, the establishment of a State Weather-Service for the especial study of thunder-storms, the discussions that helped to perfect Espy's theory of the formation of clouds and rain, the especial investigation of the New Brunswick tornado, the Meteorological Observatory of Girard College; and I should weary you were I to enumerate the many other steps in the progress of our science that I would place to the credit of the influence, direct or indirect, of that active committee. Gradually, however, its members were drawn to Washington—Henry, Bache, Walker, Espy—in succession, and at the Capital of the Nation they continued to agitate the importance of the study and the possibility of a practically useful weather-service. From 1842 until his death in 1860, Espy was untiring in his advocacy, in season and out of season, of the possibility of storm predictions and his four Meteorological Reports, as published by the National Government, constitute a lasting monument to his industry and enthusiasm. Whoever shall write a comprehensive life of Espy will sketch the progress of meteorology from its ancient to its present, and apparently even its future, condition. While

[41] JOURNAL OF THE FRANKLIN INSTITUTE, Vol. CXXIII, No. 2, Feb. 1887, p. 115 ff.

speaking of historical matters affecting Philadelphia and the Franklin Institute, let me add that the great State of Pennsylvania has also, through Prof. Coffin, of Easton, given us that great work 'The Winds of the Globe,' and to crown all, has given us William Ferrel, who has been, not merely the great expounder and developer of Espy's views, but in all mechanical questions has been to meteorology what Sir Isaac Newton was to astronomy. Espy, Redfield, Coffin, and Bache did what they could by experiment and observation and general reasoning without mathematical assistance, but William Ferrel (born June 29, 1817, in the southern part of Bedford County, Pa., and graduated at Mercersburg), published, two years after Espy's death, a treatise 'On the Movements of the Atmosphere,' which has been followed by a series of important studies in the mechanics of the atmosphere, that has made his name recognized throughout the world as the leading theoretical meteorologist of the present age."

At the stated meeting of the Institute on October 20, 1886, it was decided that the oft-discussed plan of establishing a State Weather Service be referred to a special committee, to be appointed by the President, with the request that the committee make its report, if possible, at the November meeting.

Apparently it was impossible, for the report was not made until December 15th, but this investigation was so thorough in all respects that it is reproduced in full herewith,[42] representing, as it does, the principal single move which transformed the half-century-old meteorological activities of the Institute into a governmental function.

"Report of a Special Committee of the Franklin Institute to formulate a Plan for a State Weather Service.

(Presented at the Stated Meeting, held Wednesday, December 15, 1886.)

"At a meeting of the Special Committee to Formulate a Plan for a State Weather Service, held Wednesday, December 15, 1886, the following report was adopted, and ordered to be reported to the next monthly meeting of the Institute.

W. P. Tatham, Chairman

Lorin Blodget	Wm. H. Wahl
M. B. Snyder	Alex. E. Outerbridge, Jr.

"The Sub-Committee, appointed to consider the advisability of establishing a 'State Weather Service' for Pennsylvania, under the auspices of the Franklin Institute and in conjunction with the United States Government Meteorological Bureau in Washington, begs leave to offer the following report.

"Experience has proved that the practical benefits to be derived from

[42] JOURNAL OF THE FRANKLIN INSTITUTE, Vol. CXXIII, No. 1, January 1887, p. 38.

the daily forecasts of storms, cold-waves, etc., are greatly restricted, expecially in the agricultural districts, by the want of sufficient facilities for disseminating the official information promptly. Farmers, who are especially interested in these forecasts, are usually so located that the reports do not reach them in time to be available as predictions, and they are therefore of little value to this class of the community. In order to overcome this deficiency, the experiment has been tried of establishing, in several of the Western and Southern States, an auxiliary 'State Weather Service,' with volunteer observers located at available points working under a chief, residing at some central locality, who in turn is in daily telegraphic communication with the Government Bureau in Washington. By the aid of this organization, it is found that the weather forecasts may be much more rapidly and widely disseminated than has heretofore been practicable. The State Services already established, have proved of great value to the citizens, and have afforded material aid to the Meteorological Bureau, in its efforts to extend its usefulness. It is at the request of the officers of this Bureau, that the Franklin Institute is considering the advisability of fostering the establishment of a similar service for Pennsylvania.

"The Chief Signal Officer offers to aid the work, by furnishing all blank forms, upon which the reports are written, by supplying a competent instructor, and an inspector, who will visit the stations, compare and correct the instruments, collate the reports, etc., etc.

"It appears, from the statements of the officers of the Meteorological Bureau, that the State Services already established, have proved invaluable to the communities in which they are placed; and this committee is of the opinion, that a similar service would prove equally valuable in Pennsylvania, and that it is a proper subject for the careful consideration of the Franklin Institute.

"By means of the efficient 'Railway Bulletin Service,' now in operation by the different railroad companies in this state, such a system is both possible and practicable, at a very small expense. By these companies, the weather indications of the Signal Service, are daily posted at over 300 stations well located for points of distribution.

"By the 'Flag Weather Signals,' which can be displayed at these centres, and repeated or duplicated at proper distance from each other, the entire state can soon be covered with symbols, and the indications or warnings given to every locality participating in the work.

"The value of such information is sufficient, to induce the belief, that if a system were properly organized, the people would very generally aid in these displays by volunteering their services.

"While the stations of the Signal Service may be numerous enough for storm and frost-warnings and general weather predictions, they are not sufficient to determine the climatic conditions of our state sufficiently for the best scientific and practical results.

"They are mere outposts for general work, so far apart that numberless meteorological phenomena occur, that, being unrecorded, are lost for comparison and study.

"The value of systematic and continuous records of atmospheric changes cannot be over-estimated. There is not an industry in the country that could not be benefited by them.

"For this purpose, a thorough system of taking and recording observations should be organized in every county. No doubt, with proper encouragement, a sufficient number of volunteers could be found for this work, and, in a short time, such an array of climatic data might be collected, that valuable and comprehensive information might be given, concerning the meteorological condition of any part of the state.

"As above remarked, several of the states have already experienced and recognized the practical benefits to be derived from a 'State Weather Service,' and have appropriated money to properly furnish the stations with the necessary Government standard instruments, for measuring the pressure, temperature, humidity, rain-fall, wind, etc.

"This should be done by the state of Pennsylvania, as the benefits would be general.

"An appropriation of $3,000 will properly equip one station in every county; and furnish means for the monthly publication of a Weather Review, containing the tabulated reports of the observations.

"After the stations are once established, the cost of maintaining a continuous service would be slight.

"The following is a price-list of standard instruments, as published by the Signal Service:

Barometer		$30.00
Thermometers	Dry bulb	3.00
	Wet bulb	3.00
	Maximum	5.00
	Minimum	4.00
Rain-gauge		2.50
Instrument Shelter		5.00

"For the reasons herein stated, we suggest:

"That the Franklin Institute at once organize a 'State Weather Service' for Pennsylvania, having for its object the collection and collation of climatic data and phenomena, and the dissemination of the weather-forecasts, storm- and frost-warnings of the Signal Service.

"That the work of taking observations, disseminating forecasts, warnings, etc., be accomplished by volunteer service.

"That the co-operation of railroad companies, telegraph companies, telephone companies, newspapers, and others, who can materially aid in the dissemination of information, be solicited.

"That the offer of the Chief Signal Officer of the Army, to furnish a member of the Signal Corps to assist in the work, be accepted.

"That an effort be made to secure an appropriation of $3,000 from the state, for the purchase of instruments and for the publication of the observations in a tabulated form.

"That all institutions of learning throughout the state, be solicited to take an active interest in the collection of meteorological data and the study of the science, for practical application to the various pursuits of life.

"That at least one observer be secured in every county of the state, ten of whom should take and record barometric readings.

"That copies of all meteorological data relating to the state be secured and placed on file in the Central Office.

"That immediate action be taken to secure observers and displaymen, so that the work may be commenced, if possible, on January 1, 1887.

"That, if all the work suggested, cannot be at once carried out, such portions as can be done without expense, be started as soon as practicable.

"That the management of this Service be placed in charge of a committee, appointed by the Institute, with power to regulate the service according to requirements.

"At The Stated Meeting of the Franklin Institute of the State of Pennsylvania, for the Promotion of the Mechanic Arts, held Wednesday, December 15, 1886, the foregoing report was adopted and the Committee was continued, with instructions to carry into effect the plans proposed.

(Attest.) Wm. H. Wahl
Secretary

The Institute's Committee worked very efficiently. A bill was drawn up in accordance with the foregoing report. The text of the act was read at a meeting of the Institute,[43] and was approved, with the hope that it would receive the favorable consideration of the Legislature. And then it went to Harrisburg, and passed both houses of the Legislature.

Governor Beaver dictated a letter to the head of the Institute's Committee (Fig. 8), saying that he thought "the bill should become a law" but that he had not yet referred it to his official counsellors. At the bottom of the letter is the Governor's hand-written note: "Since dictating the above I have examined & signed the bill. J. A. B."

After the establishment of the Pennsylvania State Weather Service the Institute's meteorological activities were largely supervisory. Each question of policy, each technical inquiry, was acted upon by the men chosen by the Institute to represent it in such work. Under these auspices the State Weather Service continued to fill an ever-growing need in the meteorological field. At the same time, throughout the

[43] JOURNAL OF THE FRANKLIN INSTITUTE, Vol. CXXIII, No. 3, March 1887, p. 247.

country there was a growing amount of activity in organized weather study.

The Army Signal Corps' meteorological service, designed primarily for the protection of shipping rather than for the making of weather forecasts, was continuing to expand, depending for its collection of data upon the work of independently organized observers everywhere. The forms which were being used were still those which had been developed

COMMONWEALTH OF PENNSYLVANIA

EXECUTIVE CHAMBER

Harrisburg May 13th, 1887

W. P. Tatham, Esq.,

Philadelphia, Penna.

My dear Sir:

Your letter of the 11th instant, relating to the bill providing for a State Weather Service in Pennsylvania, has been received. The bill has passed both houses of the Legislature and is now in my basket awaiting executive action. Bills are now coming to me so rapidly, however, that it is impossible for me to deal with them at once. As at present impressed, I am inclined to think that the bill should become a law, but have not yet referred it to my official councellors, with whom I consult in regard to matters of this kind. I hope our views may be in accord when final action is to be taken upon it, and think the service proposed can be made very useful to our people at large.

Very truly yours,

James A. Beaver

Since dictating the above I have examined & signed the bill

J. A. B.

FIG. 8. Letter from Governor James A. Beaver to the chairman of The Franklin Institute's Meteorological Committee, with his hand-written foot-note, announcing that he had just signed the bill which made the Institute the directing agency in the new Pennsylvania State Weather Service.

from the Institute's prototypes. The value of weather knowledge became more and more widely recognized. Finally it was decided that all the weather services should be centered in one independent governmental agency. Thus the United States Weather Bureau came into being, on the first day of July, 1891. It was a part of the Department of Agriculture. Correspondence between the Institute's Committee and the new Weather Bureau indicates the extent to which the Institute's long practical experience was drawn upon in the organization of the new Federal agency. (Fig. 9.)

With the centralization of meteorological activity at last in the form which had long been visualized by the Institute, there was no longer any necessity to carry on the separate activities of the State Weather Service. At the conclusion of the work of the Committee which was then in process, the Institute relinquished all formal contact with the new bureau.

For a number of years the only active interest displayed in meteorology at the Institute was the presentation of a lecture or the discussion of some new point of meteorological theory. There was no requirement for local action to see to it that an adequate weather service was available. This was being handled automatically.

Here and there, in the next score of years, can be found references to the construction of new instruments, speculations upon isolated meteorological observations and interpretations. There were few meteorological highlights, however. That is, not until 1912.

In 1912 Dr. R. B. Owens, secretary of the Institute, invited Dr. William J. Humphreys, of the United States Weather Bureau, to lecture before the Institute on the "Physics of the Atmosphere." This he did, in January, 1913. His paper was published in the Journal—the first of a series which attracted a tremendous amount of interest.[44] So many people expressed a desire to have Humphreys' material expanded and available for general use that the Institute decided to publish it in book form. This was done, under the imprint of The Franklin Institute, with the copyright in the name of the Institute. In the introduction, Dr. Humphreys wrote:

"The physical phenomena of the earth's atmosphere are exceedingly numerous and of great importance. Nevertheless, the explanations, even of those well understood, still remain scattered through many books and numerous journals. Perhaps this is because some of the phenomena have never been explained, and others but imperfectly so, but however that may be, it is obvious that an orderly assemblage of all those facts and theories that together might be called the *Physics of the Air* would be exceedingly helpful to the student of atmospherics. An attempt to serve this useful purpose, begun in a course of lectures at the San Diego

[44] JOURNAL OF THE FNANKLIN INSTITUTE, Vol. CLXXV, p. 207.

W. P. TATNALL, OBSERVER.

PENNSYLVANIA STATE WEATHER SERVICE,

UNDER THE DIRECTION OF THE
METEOROLOGICAL COMMITTEE OF THE FRANKLIN INSTITUTE
COOPERATING WITH THE
UNITED STATES WEATHER BUREAU
OF THE
DEPARTMENT OF AGRICULTURE.

ADDRESS H. L. BALL, [redacted] WEATHER BUREAU, [OBSERVER IN CHARGE.]

Philadelphia, Dec. 14th, 1892.

Dr Wm. H. Wahl,

Secretary,

Franklin Institute, Philadelphia.

Dear Sir:

I have the honor to request you to call a meeting of the Meteorological Committee to consider the advisability of adopting the original record forms for Voluntary Observers of the Pennsylvania State Weather Service proposed by the U.S. Weather Bureau.

As the change, if adopted, would take effect on Jany. 1st. prox., I would suggest an early meeting of the Committee.

Very respectfully.

H. L. Ball

Assistant Director.

FIG. 9. Letter asking the Institute to take executive action upon the adoption of original weather observation forms by the government service.

Aviation School (Rockwell Field) in 1914, led to the production of the following chapters—revised and reprinted from the Journal of The Franklin Institute, 1917, 1918, 1919, 1920." [45]

Almost a quarter of a century later, "Physics of the Air" is still the classical reference book of American meteorologists. In the past twenty-four months it is probable that many more books on meteorology have been published than in the preceding twenty-four years. Dr. Humphreys' work, now in its third edition, has never been supplanted in the esteem of weather-minded students, be they professionals, amateurs or laymen.

A century after the Institute had begun its activities in the field of meteorology, it might have been logical to review that span of time briefly, and to recognize that from the Halls of the Institute had come organized observational methods, synoptic maps, broad theorizing on various phenomena. There had come leaders in the infant science who carried their Institute training to wider fields. As a result of the work of The Franklin Institute there had been set up a precedent for governmental underwriting of meteorological research, there had come widespread networks of cooperating observers, using standardized methods. Far-sighted discussions of aerial transportation had been held and practical work in upper-air observations had been accomplished. Theories had been advanced, studied, exploded, altered, and given publicity to the technical world. A compendium of meteorological knowledge by a master had been first given to all students. It had been a fruitful century.

But meteorology was still pretty much of a science for the initiate. Weather might be interesting to everybody, but the idea of letting everybody try to understand the weather simply had not been brought up so nothing was done about it.

But the question did arise.

When the Institute's great new Museum of popular science was planned, undertaken and finally opened to the public a little more than a decade ago, there was no provision for a group of exhibits devoted to meteorology,—although many people, many times, said that such exhibits really had a place in the Museum. This was true,—the reasons were obvious. The most significant argument in favor of a weather display was the fact that the science of the weather is a direct application of fundamental physical principles—such an application as is unquestionably a part of the Museum's field. The instruments used in weather observation are, themselves, devices which would make good exhibits, particularly if in operation. There is no field of science which has more direct and more constant impact upon the consciousness of the public than meteorology, and the public interest in the weather is too well known to require emphasis.

[45] "Physics of the Air," William J. Humphreys, 1920, p. v.

Those meteorological instruments which did come to the Institute, instead of being held together, were scattered here and there in the sections where they might logically be placed as individual exhibits. Thermometers went to the Hall of Physics, as did barometers. It is in the Hall of Physics that the No. 1 barometer referred to on page 283 is on display. An aneroid barometer was placed in the marine transportation section. Recording thermometers and hygrometers were placed in the exhibit of building machinery, a sunshine recorder in the Hall of Astronomy. A radiososonde was briefly exhibited several years ago as a tool of cosmic ray research. Weather maps which came to the Institute went to the building superintendent, the accounting office and the astronomy department, but none went on display. The Institute, although attempting to popularize science, was for a good many years not as weather-wise as it might have been.

For a few weeks, in the latter part of May and the early part of June, 1938, a number of weather instruments were brought together and placed upon display as a feature of the Hall of Marine Transportation in connection with the dedication of the new building. These devices, lent by the Weather Bureau, occupied one table only, but they attracted a vast amount of attention, and for some time after they had been returned, after the dedication, inquiries were received about the weather instruments.

Whenever the point was raised about the establishment of a weather section, it was freely acknowledged that it was something that *should* be done, but three very potent reasons were advanced to explain why nothing *could* be done: (*a*) no space was available for a new department; (*b*) no member of the staff was available to do the work of organizing, collecting and displaying exhibits; (*c*) no money was available either to provide assistance or to purchase or make exhibits. The validity of these objections was acknowledged, but an effort was made to get around them.

In October, of 1940, the project was once more brought forward, and the specific possibility of a weather section was discussed.

A survey of virtually every square foot of floor space showed that, in truth, there was no substantial area which could be turned over for a new department.

About the only space which seemed to be doing less than a saturation job in the display of exhibits was the narrow corridor on the Twentieth Street side of the Institute which led from the second mezzanine elevators to the Hall of Astronomy and the Observatory. Only nine feet wide, it was scarcely an ideal exhibition room. There were some points in its favor, however. It was vacant; the walls were blank. It was being used as a passageway, but it *was* being used—more, perhaps, than almost any other corridor in the building, for it was open whenever the Museum was open, and also on all clear evenings. It was close enough

to the roof to make instrumental installations possible. It was not out of question as a Hall of Weather. After all, it *was* a hall.

The next step was to get some indication of approval of the project from Institute officials. If the corridor were to be accepted as the site for the projected meteorological department, and if the labor for the solicitation and organization of the new section were volunteered, if the exhibits were either donated or lent, and if the project were placed upon a basis requiring a very small expenditure in cash for materials—would the idea then be suitable?

On this basis, further inquiries were authorized. Officials of the Philadelphia office of the Weather Bureau were consulted, and eagerly agreed to lend whatever cooperation was in their power, in order to bring such a department into being. During the last week of 1940 the regular meeting of the American Association for the Advancement of Science was being held in Philadelphia, and also the meeting of the American Meteorological Society. On the evening of December 30, Commander F. W. Reichelderfer, Chief of the United States Weather Bureau and then president of the American Meteorological Society, and Dr. Charles F. Brooks, director of the Harvard Blue Hill Meteorological Observatory and secretary of the American Meteorological Society, paid a visit to the Institute. By the light of a flashlight they were shown an empty corridor, and then they sat down. The situation was outlined to them so that there could be no possible misunderstanding. The Institute had no money to expend on the project, but it did have an idea. The question was: should the idea be dropped because of the current lack of funds? The reasons for wanting to establish a meteorological department in The Franklin Institute required little exposition—to leaders in the science, no explanation was necessary. Both men expressed their earnest approval of the idea, and said that they'd be willing to lend whatever cooperation was possible, both in the planning and development of the section. They made specific suggestions, but pointed out that they were unable to take any definite step until such time as the Institute formally established the new department and requested their assistance.

With these facts in hand, early in January, 1941, the Institute formally organized the Department of Meteorology, as of the first of the year, and authorized the pursuance of the plans to inaugurate a Hall of Weather. Outlines of the desirable exhibits were drawn up, and a further conference was held with Commander Reichelderfer and Dr. Brooks at a meeting of the Institute of Aeronautical Sciences later the same month. This was followed by a visit to the central office of the Weather Bureau at Washington, where the officials of the Instrument Division were given authority to cooperate with the Institute in its plans.

The original plans called for a series of exhibits, none of which could

be very bulky, because of the space limitations. The intention was announced of dividing the exhibits into three inter-related sections: the theory of weather, the instruments used for observations, and the preparation of weather maps. This was studied and detailed, and two other categories tentatively added: the historical background of present weather science, and the application of weather knowledge to modern industry and daily life.

Sketchy plans were drawn for a number of exhibits. The part they would play in the ensemble was discussed. In the discussion of the physics of the air it was felt that not only should there be a mercurial barometer and an aneroid, but also barographs and water barometers, to show how air pressure could be measured with different types of instrument. In addition, to explain the difference between the weight of air and its pressure, a large wooden box was built by the shop students of the Woodrow Wilson Junior High School. The legend on the box indicates that it contains 13 cubic feet of air, the weight of which, under normal conditions, is one pound. However, there is acting upon the surface of the box a total pressure of 72,480 pounds, which would tend to crush it if there were not a similar pressure on the inside. The cost of this exhibit was $1.24, for lumber and paint.

Another exhibit, made completely by a volunteer, at no cost, was a circular steel rod of one square inch cross-section, cut off to such a length that its weight is 14.7 pounds. In its specially constructed stand, with two discarded rubber heels in the bottom as a cushion, this popular exhibit invites the visitor to "lift this rod." The weight is the same as the weight of a column of air of the same diameter, reaching from sea level to the top of the atmosphere.

Photographs of clouds and weather phenomena were collected, and clippings from newspapers and magazines. Some were photostated and mounted, others were filed. A special trip was made to Washington with a station wagon, and a capacity load of "rattlingly" good instruments was brought back. The exhibits were not, by any means, ready for display. They were all surplus equipment, many of them obsolete, most of them not even in operating condition.

Charles L. Dannheiser, one of the officials of the Philadelphia Airport Weather Bureau Office, took a look at the instruments and then did something about them. He took them to his home one by one, took them apart, repaired or rebuilt and refinished them, so that the end product was a collection of attractively displayed and operating devices.

Many exhibits simply needed cleaning. All needed labels. Weather maps had to be filed and sorted. To aid in this part of the job, the help of the Junior Committee of the Institute was enlisted, and this group of young women, and the husbands of some of them, rendered yeoman's service in getting the equipment in presentable shape. Polishing rags and paint brushes were the principal utensils.

While this preparatory activity was under way, announcement of the plans for the proposed section were published for the benefit of members of the Institute.[46] Since the author of that article and this one are the same person, the use of the same quotation from Benjamin Franklin to head each will not be misunderstood. It was pointed out that 1941 was an ideal year in which to open the Hall of Weather as an independent department, since it marked the fiftieth anniversary of the establishment of the United States Weather Bureau as an independent governmental agency.

This was the first public announcement of the Institute's plans. Mr. Samuel Fels, donor of the Fels Planetarium, expressed much interest in the project, and urged that it be put into effect, because he believed that it would be among the most popular sections of the Museum. He heartily approved the action which had already been taken, and in order to make it possible to carry the project through, he offered to help with defraying a portion of the incidental costs. His contribution was matched by another friend of the Institute, and the Museum, itself, appropriated a small sum, so that there might be some budget on which to draw for materials, supplies and necessary expenses.

At a meeting of the American Meteorological Society held in the Department of Commerce Building, in Washington, on April 30, 1941, the plans for the development of the "Hall of Weather" as a meteorological museum of unprecedented scope were explained and discussed. A number of concrete suggestions were made by meteorologists, and numerous offers of cooperation were received. At this meeting a conference was held with Lucien L. Friez, head of the Julien P. Friez and Sons Division of the Bendix Corporation of America—the largest manufacturers of meteorological instruments in this country if not in the world. Mr. Friez agreed to contribute a number of modern instruments to the proposed display, and volunteered to make it possible for Mr. Sterling P. Fergusson, of the Blue Hill Meteorological Observatory, to come to Philadelphia to help in the setting up of the exhibits. Mr. Fergusson is the inventor of many pieces of standard meteorological equipment a large number of which bear his name, and he is one of the world's outstanding authorities on meteorological instruments in general. In addition he had been in charge of the Weather Bureau's Division of Tests and Repairs for some years, and was formerly a professor of meteorology at the University of Nevada. He had organized a number of exhibits of meteorological equipment in the past, and Mr. Friez brought him to Philadelphia to help in the establishment of the Hall of Weather. He spent a week at this task, checking the operation of equipment, its display, and the preparation of labels.

Mr. Fergusson brought with him a number of exhibits which were lent by the Blue Hill Observatory. These tied in with the Weather

[46] *The Institute News*, Vol. V, No. 10, February 1941, p. 1.

Bureau apparatus and with pieces of equipment given by Mr. Friez. From assorted sources came other exhibits to enable a complete story to be told.

On the peak of the roof was erected a temporary instrument tower, carrying an anemometer, wind-vane, and sunshine recorder. Nearby was located a tipping bucket rain gage. These were connected electrically to a so-called triple register, which makes a minute-by-minute graph of the direction and velocity of the wind, the duration of sunshine, and the duration and intensity of rainfall. There is also a visual wind direction and velocity indicator, attached to the same exposed instruments. The Weather Bureau has made available to the Institute a 40-foot instrument tower which it is planned to erect on the roof at some later date. At this time a wider assortment of instruments will be put into operation, and, in addition, the traditional weather forecast flags will be flown. For many decades, when there was no dream of radio broadcasts and when the only means of announcing forecasts was in the newspapers, weather bureaus made it a practice to fly flags each day, so that passers-by could observe the current forecast.[47] A white flag meant "Fair," blue stood for "Rain" or "Snow." "Local Rain or Snow" was indicated by a flag, half blue and half white. A white flag with a black square forecast a "Cold Wave." A black triangular flag, flown above, meant that the temperature would rise, the black triangular flag below was a prediction that the temperature would drop. The absence of the black triangle meant that there would be no change in temperature. A number of years ago the practice of flying these flags was discontinued, but it is believed that it was a tradition which may well be perpetuated as a significant means of making people weather-wise.

The Hall of Weather was finally opened with a preview on Friday, June 6th, 1942. Officials of the Institute and representatives of both the Philadelphia and Washington offices of the Weather Bureau joined in the first viewing of the brand-new section, which, at least, had light and color and motion in it. The members of the Junior Committee, after having helped with the physical job of getting the exhibits ready

[47] As far back as May 1887, Professor Edwin J. Houston, of Central High School, spoke before the Institute concerning the flag signal code. JOURNAL OF THE FRANKLIN INSTITUTE, Vol. CXXIV, No. 2, August 1887, p. 126.

"The Committee repeat the expression of the hope that the people of each county will organize for the purpose of flagging their county so as to convey promptly to all the farmers, the daily forecasts of the weather for the next twenty-four hours. A little reflection will show the necessity for this. The repeating stations do not repeat the signal for their own benefit, but for that of the people beyond them who, in turn, pass their signals to their more distant neighbors.

"Where some do the work and others reap the benefit, a system of proper compensation is necessary, and this can be obtained only by organization." This request was signed by the Institute's Committee on Meteorology, and was appended to the Pennsylvania State Weather Service *Bulletin* for October, 1887. JOURNAL OF THE FRANKLIN INSTITUTE, Vol. CXXIV, No. 6, December 1887. Supplement.

for display, acted as hostesses in introducing the new department to the public.

The following day a group of Boy Scouts came to the formerly blank corridor to check their qualifications for a Weather merit badge. Early the next week a group of forty boys from Girard College visited the section with a teacher who had been talking to them about the weather—and this was the first time that they had had an opportunity to examine closely such a large number of weather instruments. Considering both the old and the new, it has frequently been stated, and so far not contradicted, that the Hall of Weather has the largest collection of weather instruments in the country—certainly larger than any single weather bureau office. This is despite the fact that, during the past few years, several instruments which were lent by the government have been lent back to the government, together with other equipment which was a gift to the section. During the period when weather bureaus were operating on a military-secret basis, it was acknowledged by Weatherman Henry P. Adams that it was highly helpful to tell the public that the Institute's display would be able to take care of the queries which traditionally are directed to the Weather Bureau.

A cabinet of historical data, containing some of the documents referred to in this paper, was placed near the portal of the Hall of Weather. Crude but effective charts, prepared by students in several schools, showed the structure of the atmosphere, the characteristics of air masses. A balloon meteorograph hung down in its wicker basket, while a cabinet of radiosondes showed the more modern type of upper-air apparatus. A tipping-bucket rain gage was placed so that it could be tipped by eager school children, and grown-ups still puff at a small anemometer to see how hard they can blow. Storm warning flags were placed to line the corridor wall, a ceiling light projector stands in the corner, while a clinometer is there to be squinted through by thousands. A climatological map with push buttons and flashing lights was another creation by the boys of Woodrow Wilson Junior High School, and one of the Weather Bureau's few remaining glass maps occupies a place of honor.

A mural panel was designed and executed by the students of Springside School to show vertical pressure gradient; another to show several cloud forms was built by several lower school girls from Agnes Irwin School. A model of a storm, designed by the Weather Bureau, gives a three-dimensional picture of a two-dimensional weather map (Fig. 10). An automatic lantern-slide projector showed a series of cloud pictures.

No one, least of all those who have actually worked on the Hall of Weather, has any thought that the present situation is satisfactory from a long-time point of view. Plans are already made for making substantial changes when the time is ripe, when space is available, and when all other factors will combine to make it possible to do an even better

job of telling a complete story. There are drawings of dioramas which will show the development of meteorological phenomena, push-button exhibits to set in operation the physical processes involved in weather. There will be animated weather maps—and a complete series of instruments, showing the mechanical principles which make them work. There will be exhibits which show the value of weather science in industry, in transportation, and in the home.

In the meantime, hundreds of thousands of people have seen the present display, and have gained a little better knowledge of the weather than they had before. Current weather maps are once more being shown, and the amount of interest indicated in the subject is ample justification for the efforts which have been expended in bringing the Hall of Weather to its present state.

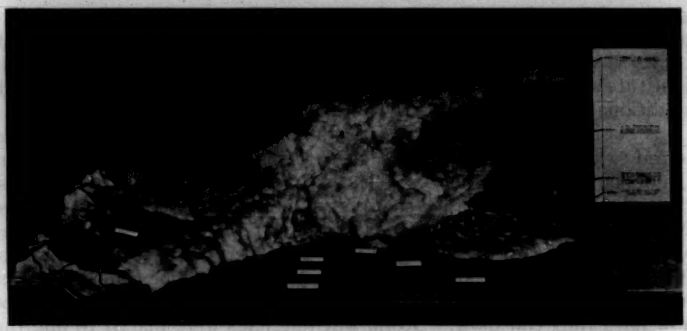

FIG. 10. A model of a cyclonic storm, showing the interaction of various types of air mass to create weather phenomena. This model was built by the U. S. Weather Bureau to be exhibited at the Treasure Island Exposition at San Francisco. It is now being displayed in the Hall of Weather.

But, while the Museum's Hall of Weather has been the outstanding connection of The Franklin Institute with meteorology today, it is by no means the only one. The Institute is an accredited cooperative station of the Weather Bureau, which primarily means that observations made here are given the same status as those of other similar stations, and are published regularly as the reports of the Franklin Institute station.

When George S. Bliss retired as head of the Philadelphia office of the Weather Bureau after having been official in charge for thirty-four years, a testimonial dinner was tendered him by representatives of Philadelphia business, industry and public service organizations, the group sitting at the feet of B. Franklin, the first of the local meteorologists.

The Philadelphia Seminar of the American Meteorological Society was organized in the Institute, and holds its meetings in the Institute. The activities of this group have recently been resumed after a necessary lapse because of war conditions but the present roll includes weather bureau officials and employees, Army and Navy personnel, college professors, private pilots, boatmen, business men, industrialists and hobbyists.

When Philadelphians want to know the difference between hail and sleet, or frost and hoar-frost, or the climatic conditions in New Guinea, they have grown into the habit of calling The Franklin Institute. Schools bring students in to demonstrate what has been taught, and the checking of privately owned instruments is an almost daily occurrence.

In addition, for several years there has been conducted in the Institute a class in Meteorology and Aerology, under the auspices of the Air-Mar School of Navigation. Several hundred students have taken this course. Among those to complete it are several who are now weather bureau employees, there are meteorologists in each of the services in all parts of the world, and several are teaching the subject in training schools.

Last fall a course in Applied Meteorology was added. This is taught by Mr. Walton Clark, Jr. of New York, son of the late Walton Clark, formerly president of the Institute.

The Franklin Institute is attempting to keep alive the meteorological activity of a century and more ago. It is making "weather-wise" people out of those who would otherwise be "otherwise." [48]

[48] It is well recognized that there are many gaps in this story of the part The Franklin Institute has played in modern weather knowledge. Any help in filling these gaps, so that the Institute's records will be complete, will be very much appreciated. A. N. S.

ARMY AND NAVY NOTES.

Chance Discovery Useful.—(*Compressed Air Magazine*, Vol. 49 No. 3.) When experiments were being conducted eighteen years ago with natural rubber latex as a material for water-proofing raincoat fabrics, pinholes of almost microscopic size mysteriously appeared after the cloth had been heated to dry it. Investigation revealed that the holes were caused by miniature explosions of air that had been trapped within the mesh of the fabric. The heat caused the air to expand and to break through the latex film that covered the cloth.

A way was found to prevent this, and satisfactory raincoat material was produced. At the same time, laboratory technicians of the United States Rubber Company were fascinated by what they had learned and conducted further experiments to determine how many holes they could create in a latex film. Eventually they succeeded in blowing as many as 6400 per square inch in sheets up to 42 inches wide and 20 yards long. Some one suggested that the sheets with thin millions of minute perforations could be used as filtering media, and that is what is now being done. The product is known as multipore, and its field of service is being continually expanded.

Fruit juices shipped to our fighting units are filtered through multipore, which is also indispensable in the preparation of blood plasma. It serves in a new process for silver-plating airplane engine bearings that has cut down the time requirement from 6½ to 1½ hours. Production of soft coal in individual mines has been increased as much as 300 tons a day by filtering out dust that was formerly lost with the processing water. Further, the material is successfully used by a steel company in connection with the concentration of iron ore by the flotation method.

The present method of making multipore is to spread latex on a cured rubber blanket in which the desired number of microscopic pits has been produced, the air trapped in each pocket expanding and puncturing the latex. These filter screens are manufactured of either hard or soft rubber and may be compounded to resist abrasion, high temperatures, alkaline and acid solutions, as well as some oils and greases.

R. H. O.

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THE INDUSTRIAL LOT AND ITS SAMPLING IMPLICATIONS.

BY

LESLIE E. SIMON

Colonel, Ordnance Department, Director, Ballistic Research Laboratory, Aberdeen Proving Ground, Maryland.

ABSTRACT

It is shown that wrong inferences are drawn from samples because of failure to understand what constitutes a lot and because of failure to verify the existence of a lot in the practically useful sense. Time-tried methods of making valid inferences from samples without increased cost are pointed out.

The General Concept of Lot. I would like to present for consideration some observations on what is known in commerce and industry as the lot. It may seem strange that such a well-known term—a term in such common usage—should merit special discussion. Engineers, business men, and production men may feel that they know quite well what a lot is, a sub-lot, or a batch, that the terms are flexible, and of no great importance. Statisticians may feel equally confident, and dismiss the matter at once as a kind of layman's substitute for their precisely defined concepts of universe or population.

Lot size is generally regarded as unimportant, if a property of the lot at all, yet it is the keynote of skepticisms which sometimes arise regarding implied properties of the lot. For example, a lot must have a size, and the maximum size of lot is generally limited in some way; e.g., by specifications. In this connection, strong language is sometimes used against the limit of 20,000 to a lot, which is so common in Government specifications, on the grounds that if the figure is economically right for a certain type of article, such as a fuze, it almost certainly cannot be right for almost everything else. It has been suspected that there must be sound logical grounds upon which lot-size could be based, and there are. I believe that only a brief discussion is needed to show that incomplete concepts of what is implied by the word, lot, are responsible for innumerable misunderstandings in business transactions, for economic losses in industrial work, and for a great deal of misdirected effort on the part of industrial statisticians.

Uniformity is generally assumed to be in some way associated with lot. This property is so important to munitions that methods predicated on the concept of Quality Control have been used to a considerable extent to insure uniformity within the lot and to avoid the costly confusion which can arise from improper identification of what constitutes a lot. This experience has demonstrated that the general confusion about lots is a matter which admits of correction quite readily, and in such a way as to remove both the existing ills and losses and at the same

time improve the quality of the products to which the clarified term is applied.

Webster's unabridged dictionary defines lot in the commercial sense as "an article, or parcel of articles, offered as an item for sale." As might be expected, this definition reflects rather accurately the generally accepted concept of lot. It is nothing more or less than an aggregation of articles. If the articles were produced more or less continuously by a well controlled mass production process, the lot may be a mere division with respect to time. There would be no harm in this loose and naïve concept of lot were it not for the fact that a great many people who have not thought very seriously beyond this stage of the concept proceed to imply many more things about the lot.

The Implication of Homogeneous Lot. For example, lots of articles are generally accepted by the purchaser on a basis of sampling inspection. As soon as one speaks of samples there is an implied relation between the sample and the remaining unsampled portion of the large lot. If a relation did not exist, it would be futile to inspect the sample for the reason that no conclusion could be drawn with regard to the lot. In other words, one may simply fail to raise the question of the possibility of the total absence of homogeneity within the lot.

The tacit assumption of homogeneity seems especially strange in view of the fact that even the humblest sampler seems shrewd enough to have at least some concept of dispersion. His native intelligence tells him that no two articles are precisely alike and that perhaps one part of the lot may be different from another. Consequently, he tends to demand what he often calls a random sample, or at least a sample all articles of which do not come from the same portion of the lot. This second inference about the lot implies not only the relation between sample and lot, but further implies an ordered and at least knowable relation between different proportions of the lot. If the difference between articles sampled is too great, the purchaser may reject the lot on the grounds that the relation between its component articles is too loose even though the average of the articles sampled may be satisfactory. Strangely enough it does not occur to him to make a test to see whether there exists any relation at all between various portions of the lot.

Thus it is evident that, as used industrially and commercially, the term, lot, implies far more than a "parcel of articles." It must further imply that the lot is some sort of entity yet to be defined in which an ordered and knowable relation exists between the characteristics of portions of individuals which compose it.

The Statistician's Error. Whereas the layman's concept of lot or batch is much too loose, the statistician may make quite as grave an error in the opposite direction. The drawing of erroneous inferences from scant data is an anathema to him. He sees with crystal clarity the mote in his brother's eye, but let us for a moment examine the beam which he may be harboring in his own eye. When faced with a problem

involving samples and lots the statistician is apt to identify the industrial lot with his own concept of universe or population. Thus he may regard his problem as quite elementary and gayly proceed to apply his various statistical techniques, ranging from exact probabilities associated with multiples of the standard deviation in the normal distribution, to tests of randomness of sample. He as well as the layman may quite neglect to test for the existence of any universe at all.

The Importance of Order. As far as the single lot is concerned, often there is nothing that the statistician could do about it even if he realized his mistake. Given a small number of sample items, taken at random from an alleged universe, there is in general no way under the shining sun that he can determine if they are from one universe or from two or more universes which do not differ by an overwhelming amount. The difficulty exists for the greater part because of one reason only: because *order has been lost.* That one piece of essential information [2] would make the difference between almost no knowledge and a great deal of knowledge. If the samples had been taken in some purposive order; e.g., at approximately equal intervals with respect to time during the production of the alleged lot, a great deal could be said about the singleness of universe and about the measures of its quality. However, it is also noteworthy that under these circumstances one would no longer be regarding the alleged lot as a lot, but rather making sub-lots from fractions of the alleged lot.

A Newly Expressed Definition of Lot. Let us examine briefly a concept of lot which can at least have a practical and useful purpose and have an operationally verifiable meaning. I venture a definition: *a lot is an aggregation of articles which are essentially alike.* That definition as it stands might satisfy the layman, but in order to make the definition operationally verifiable, some will demand that the phrase, essentially alike, be clarified. Let us then add: where essentially alike means that small sub-groups of sample items taken from the lot in arbitrary order will respond to the Shewhart [1] criterion of control.

It will be shown later that this definition of lot closely parallels the layman's vague and unvoiced concept of lot and the statistician's frank evaluation of its composition. It is hoped to show further that a general acceptance of this concept could achieve a large economic gain for our industrial economy. However, before doing so, let us examine briefly the manner in which a lot comes into being.

The Birth of a Lot. Let us for a moment consider a lot of piece parts made by a single automatic screw machine, where the diameter, X, of the piece part is an important quality characteristic. If there were no tool wear or other systematic or sudden changes in conditions, variations in diameter might be caused merely by slight random variations in the quality of flow of the cutting oil, minor variations in the raw material, slight changes in the ambient temperature of the room, and a large number of other causes having a small and variable effect. Since it is

known that the combination of a large number of random variables results in a normally distributed variable, the quality characteristic, diameter of piece-parts, might under these conditions be normally distributed. However, these conditions do not obtain in practice. There is tool wear which progresses as a function of time and consequently the individuals of at least part of the distribution are not normally distributed about some mean value, $\bar{X}'$, where

$$\bar{X}' = \frac{\sum_{i=1}^{N} X_i}{N},$$

and N is the number of articles in the lot or universe of discourse. Instead, the individuals may be regarded as being distributed about a mean which is systematically shifting. Furthermore, the raw material is not entirely uniform but consists of discrete bars of stock whose quality characteristics with respect to machinability are likely to vary by discrete jumps from bar to bar. Therefore, the resultant lot of piece parts cannot be a smooth homogeneous universe or population of any describable form but instead its distribution consists of something like a broken line made up of short curves.

Now consider a lot of piece parts coming not from a single automatic screw machine but from several automatic screw machines. Even the initial settings of the machines could not have been exactly the same. Hence, there results a combination of sub-universes, each varying about a different mean value, $\bar{X}_1'$, $\bar{X}_2'$, $\cdots$. If the number of automatic screw machines were very large and the differences in the $\bar{X}'$'s small and randomly distributed, Mathematical Statistics tells us that the resultant distribution might again approach the well-known normal universe. However, in practice, the number of machines is not likely to be very large and the differences may be neither very small nor randomly distributed. In like manner, the variation associated with the respective machines might be different as measured by the standard deviations, σ_1', σ_2', $\cdots$, of the diameters of their respective products of piece-parts. Under these circumstances one may certainly take a sample of n articles, compute the observed average, $\bar{X}$, where

$$\bar{X} = \frac{\sum_{i=1}^{n} X_i}{n},$$

and the sample standard deviation, sigma,

$$\sigma = \sqrt{\frac{\sum_{i=1}^{n} (\bar{X} - X_i)^2}{n}},$$

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but certainly no statement can be made regarding the precise percentage of the lot which will be expected to fall within plus and minus any multiple of sigma.[1] That is a part of what Shewhart [1] means when he says that the 3-sigma limits in the criterion of control are not predicated on any value of probability but are so established because they work in practice.

The Shewhart Criterion of Control. The meaning of "working in practice" can be readily seen at this point. If none or very few of the $\bar{X}$'s, which came into being for a brief time, differ greatly one from another and if the series of standard deviations which existed from time to time, or from machine to machine, differ not too much one from another, then it is manifest that although one would have to regard the center of the distribution not as a mathematical line (without thickness), but rather as a band of finite width, and one would have to admit a degree of incertitude about the precise value of the standard deviation, nevertheless practically all of the distribution will lie within the average of any measure plus or minus 3 standard deviations of that measure. That is to say, practically all the individual X's will lie within $\bar{X}' \pm 3\sigma'$; and if random samples of size n are taken from the lot and the average, $\bar{X}$, of the sample and its standard deviation, σ, observed,[2] practically all $\bar{X}$'s will fall within $\bar{\bar{X}} \pm 3\sigma_{\bar{X}}'$ and practically all σ's will fall within $\bar{\sigma} \pm 3\sigma_\sigma$, where $\bar{\bar{X}}$ is the grand average of the $\bar{X}$'s, $\bar{\sigma}$ is the average of

[1] In this connection, Wilks [7], in a most outstanding article, has determined rigorously sample sizes necessary for setting tolerance limits to estimate the middle 100a% of the universe.

[2] Usually $\bar{\bar{X}}$ is taken as the true average $\bar{X}'$ where for k samples of size n,

$$\bar{\bar{X}} = \frac{\sum_{j=1}^{k} \bar{X}_j}{k} = \frac{\sum_{i=1}^{n}\sum_{j=1}^{k} X_{ij}}{nk}.$$

Also, σ' may be taken as $\frac{\bar{\sigma}}{C_2}$ where

$$\bar{\sigma} = \frac{\sum_{j=1}^{k} \bar{\sigma}_j}{k}$$

and C_2 is a factor to correct for the bias of sample size taken from the distribution of standard deviation.

$$\bar{\sigma} = \sqrt{\frac{2}{n}} \frac{\left(\frac{n-2}{2}\right)!}{\left(\frac{n-3}{2}\right)!} \sigma' = C_2\sigma'.$$

F. R. Helmert, *Astronomische Nachrichten*, Vol. 88, No. 2096, 122 (1876).

Tabulations of C_2 appear in references [1], [3], and [5].

$$\sigma_{\bar{X}} = \frac{\sigma'}{\sqrt{n}} \quad \text{and} \quad \sigma_\sigma = \frac{\sigma'}{\sqrt{2n}}.$$

For practical purposes, however, similar treatment of the simple statistic, *range*, is often to be preferred (see Ref. [3], [5], and [6]).

the σ's and σ_σ is the standard deviation of the σ's. This is well known from both theory and practice. That is to say, one could regard the lot as composed of members which are essentially alike in that they differ one from another by little more than that which should be expected as random variations in a homogeneous universe.

It remains only to test whether these conditions existed in practice. This test is made with classic simplicity merely by taking small samples in arbitrary order (usually in order of production with respect to time) and testing to see whether the averages of the small samples fall within plus and minus three standard deviations of the grand average and whether the standard deviations of the small samples fall within plus and minus three standard deviations of the average standard deviation. If the condition is met, the arbitrarily chosen samples appear to behave as the randomly selected samples described in the preceding paragraph. It follows, then, that the lot appears to be homogeneous in the sense that discrete parts of the lot have the same characteristics as the lot as a whole (see Ref. [3], pp. 161–163). Thus, a meaning can be given to the term "essentially alike" which is subject to operational verification by the statistician and which is satisfactorily useful in industrial practice.

Lot-Size Inherent in the Production Process. Since it is so thoroughly evident that for practical purposes *the individuals of the lot must be essentially alike*, further discussion of the criterion of control can be deferred and some evident facts brought out at once. If the lot is to be a useful category, its size must be no greater than the number of articles which a manufacturer can make essentially alike. However, it is quite impossible to tell beforehand how long a manufacturer can produce articles which are essentially alike. This will vary not only from manufacturer to manufacturer but from time to time with the same manufacturer. Hence, it is quite impossible to prescribe *a priori* any lot-size. *The lot-size and the lot itself, with all the properties which describe its quality, are determined by the production process.*

Determination of Lots. In view of the observations made thus far, one may question if it is possible to identify and make up a lot. Whereas it is not proposed to treat the subject exhaustively, this question may be answered with the general statement that it is quite practicable to determine the varying quality of the continuous flow of the product as accurately as one chooses, and concomitant with this determination to break the continuous flow up into unit categories—call them lots or anything else—upon the demonstration of a change in "essential sameness" by as much or as little as is of economic consequence. The problem is solvable in an economic way with the expenditure of no more effort than that usually expended upon an invalid assessment of the quality of the current pseudo-lot.

Consider a series of categories differing with respect to size as follows: a grand-lot consisting of 10 to 40 lots, a lot consisting of 10 to 40 sub-

lots, and a sub-lot consisting of 10 to 40 batches. Suppose further that it is possible to take small random samples (consisting of 4 to 40 articles) from the batches as they flow from the assembly or production line.

It is no more possible to judge the quality of the batch from the small random sample taken from that batch than it is to judge the so-called lot from the random sample taken from that lot. In neither case is there a check on the internal consistency of the unit collection sampled. Quality may have changed in the middle of the batch, and one does not know and cannot know whether such change has occurred. However, this may not be important for (1) the batch is a very small unit and the change, if any, is not likely to be great, (2) it will be shown subsequently that one can discover quickly a departure from essential sameness, if it occurs, and will know that it occurred at about batch number so-and-so, even though it will not be known whether the change occurred in the early, middle, or late part of the batch.

Suppose now that one observes the average value of the sample items from each batch expressed as the arithmetic mean, $\bar{X}$ (that is what is generally done), and one observes also the dispersion of the sample items of each batch expressed as standard deviation, σ; probable error, P. E. $= 0.6745\sigma$; or maximum dispersion, R, i.e., the greatest value minus the smallest value in the sample. Now, just a little statistical method must be added as a leaven to all this common sense. It can be shown statistically that if all the batches are merely subdivisions of a larger, uniform, and consistent category, two things will occur:

(1) almost all the batch dispersions will fall within the range defined by the average of the batch dispersions $\pm$ a constant [3] times the average of the batch dispersions.

(2) almost all the batch averages will fall within the average of the batch averages $\pm$ another constant [3] times the average of the batch dispersions. This is essentially the Shewhart criterion of control mentioned in the earlier part of the discussion. Thus, the occurrence of a batch average or dispersion beyond these limits marks the end of a category within which *one is justified as acting as if all the articles were essentially alike.* Furthermore, one knows the quality of this larger category, the sub-lot, quite precisely, *for its average is very close to the average of the batch averages, and its dispersion is very close to a simple multiple of the average of the batch dispersions. Thus, without a very precise knowledge of batch quality, one has a very thorough knowledge of sub-lot quality.* This procedure answers the question of determination of quality only for a special set of circumstances.

The Purchaser's Dilemma. However, for the purchaser, this procedure is entirely impracticable. It is quite impossible for him to go into the manufacturing plant and sample the batches. In general, he

[3] In practice, the mathematics of footnote [2] is generally reduced to this simple procedure.

cannot have access even to the sub-lots for sampling inspection, although, under some circumstances, this is practicable; e.g., under some Government contracts. Generally, the purchaser cannot specify the product and also govern its manufacturing process. Often he must be limited merely to accepting or rejecting the finished articles on a basis of a sampling inspection prescribed by the specification. Hence, the purchaser's samples are generally limited to the so-called lot, which may or may not be a lot in the true sense of the word, i.e., an aggregation of articles which are essentially alike. If the whole lot were available for the application of a planned sampling technique, the purchaser could, by purposive sampling, check on the homogeneity of the lot, but he could not tell where a change, if any, in essential sameness occurred because that precise piece of information, *order*, is lost in handling and shipment. The lot offered for acceptance, for example, may be composed of two or more sub-lots whose true levels differ significantly. Has, then, the purchaser any economic method of ascertaining the quality of the product offered by the vendor?

A Method of Quality Determination. There is an easy solution to this problem which under many circumstances may be almost good enough. Certainly the ideal method would be that of sampling the batches or sub-lots. Under those circumstances, there would be opportunity for locating the cause of poor quality when it occurred, and opportunity for eliminating the cause of the trouble, and thereby producing almost all good quality. That would be Process Quality Control. Whereas the door to Process Control is closed (1) by loss of order and (2) by the remoteness of the inspection from the cause of bad quality, nevertheless *quality determination* through the methods of Quality Control is entirely feasible.

It should be remembered that from the viewpoint of principle, it is not very important whether one samples the batch, sub-lot or lot. The basic principle still holds: *the valid quality prediction pertains to the echelon one order higher than the unit sampled.* If one samples sub-lots, valid quality predictions can be made about lots, for order has been preserved and a change in quality can be detected. If one samples lots, the valid quality prediction pertains to an aggregation of lots, or the grand-lot. The economic penalty simply is this: the larger the unit of valid quality prediction, the greater the quantity to be scrapped, reworked or one hundred per cent. inspected when trouble occurs, and the greater the liability of failure to detect some minor or brief quality variations.

However, these penalties may be small. In very large-scale inspection for acceptance, such as is now encountered in war production, altogether too much attention is liable to be focused on the occasional bad lot rather than on the whole group of sub-standard lots or on the bad manufacturer. If only the bad manufacturer is assuredly identified and

the flow of his product arrested before it has done much harm, much will have been accomplished.

Illustration of True Lot Identification. In Table I is given the average, X, and the dispersion expressed as the range, R, for samples of five

TABLE I

Lot	Mean Muzzle Velocity f/s	Range f/s
1*	1710	42
2*	1711	40
3*	1713	39
4	1718	26
5	1735	10
6	1739	25
7	1723	14
8	1741	15
9	1738	11
10	1725	31
11	1731	25
12	1721	19
13	1719	43
14	1735	39
15	1741	17
16	1783	51
17	1777	09
18*	1794	15
19	1773	37
20*	1789	54
21*	1798	15
22*	1789	29
23*	1788	39
24*	1799	30
25*	1807	44

* Failed to meet specification.

taken from 25 consecutive lots of a manufacturer of complete rounds in which the quality characteristic muzzle velocity [4] was measured. The lots which failed to meet the specification: $1715 \leq \bar{X} \leq 1785$, $R \leq 70$ are marked with an asterisk. This is the usual method of lot-by-lot acceptance inspection.

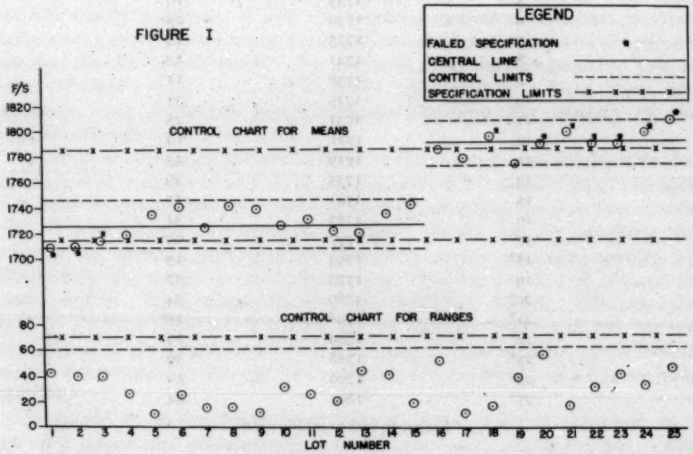
In Fig. 1 the Shewhart criterion of control is applied. It is quite evident that the first 15 lots are of essentially the same quality, and none should have been rejected. Those which were rejected (or retested until they passed) represent merely so much economic loss. However, near or between Lots 15 and 16 a change occurred. If the specification criteria are really important, all the Lots 16 through 25 (not just part of them) should have been rejected. Thus, it is all too evident that Lots 1 through 15 constitute the real lot (called the grand-lot) in the

[4] The data have been altered in scale only so as to reveal no actual military information.

sense that they are *an aggregation of articles which are essentially alike.* In a like manner Lots 16 through 25 constitute another real lot, but a lot which should not pass the specification.

The Use of Attributes. In the above discussion attention has been focused entirely on measurement by variables, i.e., on quantitative measurements. It was pointed out that only those categories conforming to the Shewhart criterion of control should be combined to form the next higher homogeneous echelon. This may be compared, on the other hand, to measurement by attributes, i.e., qualitative measurements. In the case of attributes a homogeneous lot may be formed by

FIGURE I



mixing together thoroughly lots of different fraction defective, if mixing is physically practicable and provided that one is willing to accept the consequent result. For example, consider two lots, one of fraction defective $q_1 = 0.01$ and containing $N_2 = 1000$ articles, the other of fraction defective $q_2 = 0.04$ and containing $N_2 = 2000$ articles. By mixing thoroughly the two lots we may form a homogeneous lot of 3000 articles which will have a fraction defective,

$$q_3 = \frac{N_1 q_1 + N_2 q_2}{N_1 + N_2} = \frac{(0.01)(1000) + (0.04)(2000)}{3000} = 0.03.$$

In practice there may be some cases in which the resulting fraction defective is acceptable; in other cases, however, the final fraction defective may be intolerable.

Thus, whereas the consequences of occasionally accepting a small quantity of material which is more defective than intended by the specification may be merely an adulteration of the flow of product in the sense that acceptance of N articles which are 3 per cent. defective is equivalent only to 97 per cent. of N articles which are perfect, nevertheless, it may be quite important to detect changes in the flow of a product measured by attributes and to classify portions of it by lots which contain known levels of fraction defective. The procedure for identification of lots of attributes is similar to that described for variables except that only one statistic (the fraction defective) is used and the sample size, n, should be somewhat larger. In order to be justified in regarding each increment of flow as essentially the same as its predecessors, the fraction defective of the sample from that increment should fall within

$$\bar{q} \pm 3\sigma_q \text{ where } \bar{q} = \frac{\sum_{i=1}^{k} q_i}{k},$$

and

$$\sigma_q = \sqrt{\frac{\bar{p}\bar{q}}{n}}, \quad \text{and} \quad \bar{p} = (1 - \bar{q}).$$

Other methods of setting limits are given in references [3], [5] and [6].

Sampling Implications. As a corollary to the definition of lot, it is seen also that whereas it is quite impossible to specify *a priori* how long a manufacturer can make articles which are essentially alike, it is quite easy to determine *a posteriori* how long he did make them alike. Hence, lot-sizes, in the layman's sense, should be thought of as pertaining to mere sampling units of a size which is convenient and economic (not as uniform units), whereas lot-sizes in the true sense can have no number preassigned, since they pertain to practically useful units which can be determined only after the product is inspected.

Thus, it is apparent that the layman should make a distinction between *unit* lot and *uniform* lot, that he does not or should not think of the industrial lot as "an article or parcel of articles offered as an item for sale." If he does so he is going about his business most blindly. Surely he must or should regard the lot of articles as an aggregation of articles which are essentially alike and hence subject to a sampling inspection from which an inference might be reliably drawn regarding the quality characteristics of the whole lot. Furthermore, the layman must realize that the sample from the lot does not give him certain knowledge regarding its quality characteristics but probable knowledge only. However, by intelligent use of this probable knowledge he can make the sum of the costs of his errors in mistakenly accepting sub-standard quality and the cost of his errors in mistakenly rejecting standard quality a minimum. That is to say, he can make economic and predictive use of

sampling data. In like manner, the statistician must or should realize that, in this imperfect world in which industrial lots are produced, it is generally impossible for him to make precise probability statements about the lot, but that nevertheless he can make valid statements about the essential sameness of the articles which compose the lot which are entirely satisfactory for practical purposes.

Summary. Predictions of lot quality predicated on small samples are invalid in the absence of assurance of a knowable relation between the inspected sample and the uninspected remainder. Assurance of the existence of such a relation is readily obtained by taking small samples from arbitrary divisions of the alleged lot and then testing to see if the arbitrarily chosen samples appear to behave as random samples from a universe. The valid quality prediction then pertains to the echelon one order higher than the unit sampled. There are three degrees of efficiency in making quality predictions.

First, small random samples taken from the lot at an earlier time and at a different place may be inspected. Under this condition, the quality prediction is of very little value.

Second, the whole of the alleged lot may be available for inspection; it may be divided into arbitrary strata; sampled; and the criterion of control applied. Under these conditions, a knowledge of the validity or non-validity of the prediction is available, but no efficient corrective measures are possible if the lot appears to be non-homogeneous.

Third, small samples may be taken in order of production and the criterion of control applied. Under this condition valid lot-quality predictions are possible and efficient division of the flow of production into true lots is automatically obtained without additional cost.

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EXPERIMENTS ON AND THEORY OF THE ACTION OF THE GEIGER POINT COUNTER.

BY

JOSEPH MORGAN,

Texas Christian University, Fort Worth, Texas,

AND

J. LLOYD BOHN,

Temple University, Philadelphia, Pa.

ABSTRACT.

Curves are included showing the relationship between voltage and relative position of electrodes, and between voltage and counter gas pressure. Also included are experiments on the preparation of sensitive and insensitive points, and experiments with different electrode materials. These data are for a counter operated with the point at negative potential.

Zeleny's conclusion that a high resistance layer of adsorbed gas exists at the point is adopted. In addition, the assumption is made that the field at the point at the onset of "self-counting" is sufficient to produce auto-electronic emission. This combination now leads to a theory which accounts for the various characteristics of the counter and explains the threshold of counting curve, the auto-counting curve, and the continuous discharge curve. Probability considerations account for the selective counting of different ionizing agents in certain voltage ranges, for the dependence of counting upon path of ionizing rays through the chamber, and for the increase of counts with increase of voltage from threshold of counting to the point where all rays are registered.

INTRODUCTION.

Electrical counters were first developed by Rutherford and Geiger [1] in 1908 in the form of tubes and thin coaxial wires, and in 1912 they used a counter in the form of a ball and tube.[2] The point-counter, with which the present investigation is concerned, was introduced by Geiger [3] in 1913, and since that time this type counter has been the subject of many investigations.

Attempts to coordinate the various theories concerning the characteristics of the Geiger point counter lead to many difficulties. Apparently, the individual theories satisfy the count mechanism satisfactorily under their respective assumptions, but fail to explain some phenomena which invariably occur with the counter. The counter has been extensively investigated by Geiger,[4] Zeleny,[5] Emeléus,[6] Curtiss,[7] Bennett,[8] and many others. None of these, however, has attempted an explanation of the "self-counting" phenomenon. In the present endeavor to explain the counter action and characteristics of the counter, many of the experiments that have previously been performed are repeated and new ones added. A critical survey of the results is made, a theory of the action of the counter is formulated and the various characteristics of the counter are explained in terms of the theory.

APPARATUS.

The voltage supply for the counter consisted of a transformer, mercury vapor rectifying tubes and suitable filter circuit to produce 2200 volts D.C. The amplifier was a three stage vacuum tube type. The counter was the ordinary type shown in Fig. 1 (not drawn to scale). It contained an insulating stopper fitted with a brass bushing, S, of inside diameter $\frac{1}{4}$ inch and threaded 28 to the inch. In this manner, the

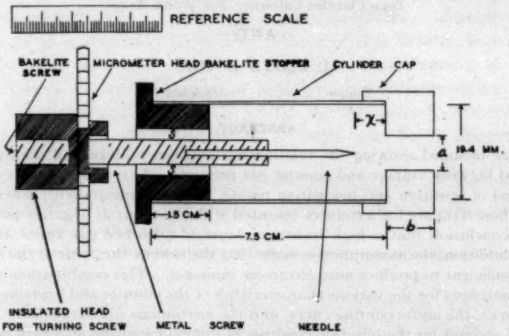

FIG. 1. Diagram of Geiger Point Counter (not drawn to scale).

advance or recession of the needle was accomplished without lateral displacement. The needle was held firmly in place by the tightly fitting hole in the metal screw. A micrometer head and reference scale served to indicate the extent of horizontal motion. The caps employed were close fitting and contained a centrally located circular hole. Auxiliary apparatus consisted of various caps, cylinders, points, planes, and suitable sources of α-, β- and γ-rays for actuating the counter.

RESULTS.

Unprepared copper points would not function for more than about one or two hours. Steel needles, ground down to fine points, operated well but for approximately the same time. A ground steel point, after use, was never sensitive on the day following unless it was freshly ground. Smooth darning needles, however, worked most satisfactorily, the only treatment being the application of a fine clean linen cloth to wipe the needle point before use. Such a needle was used to obtain the quantitative results, its sensitivity remaining good from day to day. At times, counts were not registered until after the application of the cloth to the point. If a point did not function, it was occasionally restored to working conditions by first raising the voltage high enough to cause a disruptive discharge and then lowering it to a value requisite for producing counts.

The functioning of a point counter depends on the difference of potential between its electrodes and the distance of the point from the end of the cylinder (x). Variation of applied difference of potential with (x) was investigated for many combinations of cylinders and caps. For varying distances (x) from the cap, the corresponding voltages necessary to produce about 20 clearly defined counts per minute were taken. The source of ionization was a radioactive substance emitting β-rays and, when used, was held at a distance of about 4 cm. from the centrally located hole in the cap, so that the rays entered the chamber along a line practically coincident with the common axis of point and cylinder. The gas employed was air at atmospheric pressure and room temperature. The distance (x) was varied by the screw attachment and read with the micrometer and reference scale shown in Fig. 1. The walls of the chamber were positive with respect to the point, the latter being kept at ground potential. In obtaining the potential necessary to produce 20 counts per minute for any given value of (x), the voltage was always gradually increased from a value too low to produce counts up to the necessary amount. Sample curves are given in Fig. 2 for the combination employing a flat cylindrical brass cap and brass cylinder, the dimensions of which were those indicated in Fig. 1, with $a = 5.7$ mm. and $b = 3.6$ mm. The abscissa values to the right of the origin indicate positive distances (x), the point starting at $x = 0$ and proceeding into

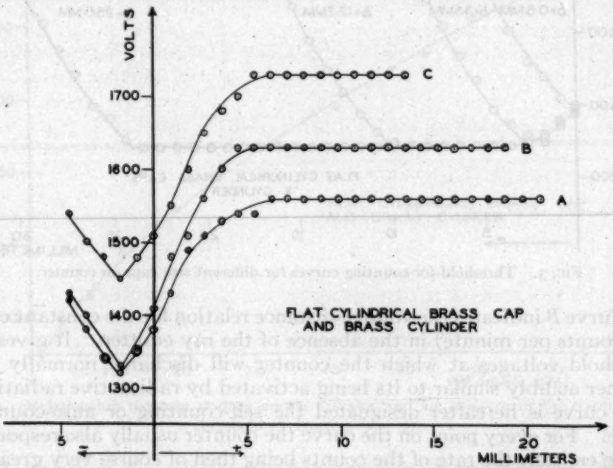

FIG. 2. Three characteristic curves for the counter. *A*—Threshold for counting. *B*—Threshold for self-counting. *C*—Threshold for disruptive discharge.

the chamber while those toward the left of the origin correspond to negative distances ($-x$), the point proceeding out from the cylinder through the centrally located hole in the cap.

Curve A, Fig. 2, shows how the voltage on the tube varies with the distance for an effect corresponding to 20 counts per minute, when the chamber was under the influence of the ray emitter. In the absence of the emitter, for each point on the curve the effect was entirely absent. When the voltage corresponding to any point on A was lowered only slightly (about 10 volts), the effect was still heard but the rate of the counts was too slow (about one every two or three minutes) for repeated observations. Curve A then yields threshold voltages for corresponding distances.

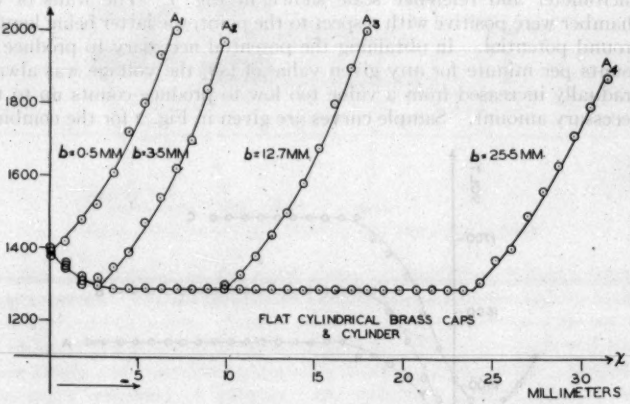

FIG. 3. Threshold-for-counting curves for different size caps on counter.

Curve B indicates the voltage-distance relation for the constant effect (20 counts per minute) in the absence of the ray emitter. It gives the threshold voltages at which the counter will discharge normally in a manner audibly similar to its being activated by radioactive radiations. This curve is hereafter designated the self-counting or auto-counting curve. For every point on the curve the counter usually also responded to the emitter, the rate of the counts being then of course very great, in fact almost continuous.

Curve C shows how the voltage varies with (x) for a continuous discharge. The presence or absence of the emitter made no difference in the effect observed. This curve merely indicates the voltages at which disruption occurred.

The figure reveals a distinct minimum for all three curves. That the general case, for caps with different sized b's, does not contain a one-

point minimum is seen in Fig. 3, where the variation of voltage with negative distance only is shown. These results were taken in the presence of the ray emitter and correspond to the negative portions of the A curves. The curves reveal that in general a one-point minimum does not occur but that when this minimum is reached there is a flat portion which continues for the greater portion of the tube length inside the cap. When the point is very near the end of this tube length, and before it emerges, the voltage increases in the manner shown by the four branch portions A_1, A_2, A_3, A_4. The convex portion leading to the minimum for A_2, A_3, and A_4 is represented by one average solid line, the separate ones being practically coincident with this one. The fact that A_1 does not show a flat portion at the minimum value may be attributed to the small value b; also, the apparent point-minimum in Fig. 2 is explained by observing that the range of this flat portion is rather small.

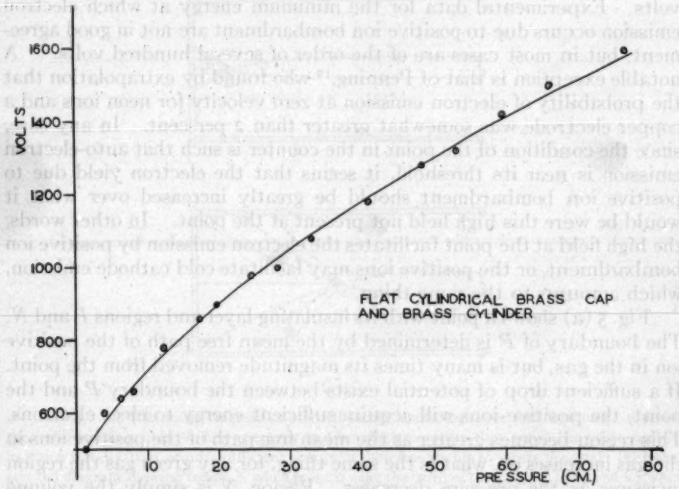

FIG. 4. Relationship between voltage on the counter and air pressure in the counter for threshold of counting ($x = 0$).

Fig. 4 shows the relationship between voltage and pressure for the threshold of counting in the presence of the ray emitter ($x = 0$).

THEORY OF THE ACTION OF THE COUNTER.

To account for the varied observational phenomena we propose a theory for the action of the Geiger Point Counter involving three assumptions:

(1) An insulating or high resistance layer at the point.

(2) Emission of electrons from the point by means of positive ion bombardment.

(3) Auto-electronic emission from the point.

That a layer of gas exists at the surface of a metal has been established by Davisson and Germer.[10] The insulating properties of such a layer have not been established by direct measurement, but the work of Zeleny [9] and others [11,12] and of the authors indicate its presence. The other two assumed phenomena do occur at sufficiently high fields.[13]

It is doubtful whether the positive ions ever receive sufficient energy (a very small fraction of a volt) in the counter to eject electrons by virtue of this energy alone. One of the lowest energy values for ionization is given by Tate [14] in the case of sodium ions at 40 volts exciting mercury arc lines in mercury vapor. In air, Sutton [15] finds no ionization at 100 volts. Experimental data for the minimum energy at which electron emission occurs due to positive ion bombardment are not in good agreement, but in most cases are of the order of several hundred volts.[13] A notable exception is that of Penning,[16] who found by extrapolation that the probability of electron emission at zero velocity for neon ions and a copper electrode was somewhat greater than 2 per cent. In any case, since the condition of the point in the counter is such that auto-electron emission is near its threshold, it seems that the electron yield due to positive ion bombardment should be greatly increased over what it would be were this high field not present at the point. In other words, the high field at the point facilitates the electron emission by positive ion bombardment, or the positive ions may facilitate cold cathode emission, which amounts to the same thing.

Fig. 5 (a) shows a point with its insulating layer and regions P and N. The boundary of P is determined by the mean free path of the positive ion in the gas, but is many times its magnitude removed from the point. If a sufficient drop of potential exists between the boundary P and the point, the positive ions will acquire sufficient energy to eject electrons. This region becomes greater as the mean free path of the positive ions in the gas increases or, what is the same thing, for any given gas the region increases as the pressure decreases. Region N is simply the volume beyond which the electrons cannot gain sufficient energy in their free flights to ionize the gas.

Let any ionizing radiation enter the counter and produce an initial group of ions. The positive ions will pass to the point and, if they gain sufficient energy in passing through region P, they will eject electrons from the point. These electrons in passing outward through region N produce more ions. If the number of pairs of ions thus produced is at least equal to the number of initial pairs of ions, the process will continue until the charge on the insulating layer due to the addition of positive

ions and ejection of electrons is sufficient to lower the field at the point so that the positive ions, in passing through the region P, can no longer acquire the required energy to eject electrons. The ionization then ceases, the charge accumulated on the insulating or high resistant layer leaks off, and the counter returns to its original condition and is ready for the next cycle. As the voltage on the counter is increased, the positive ions gain more energy in passing through region P and are more

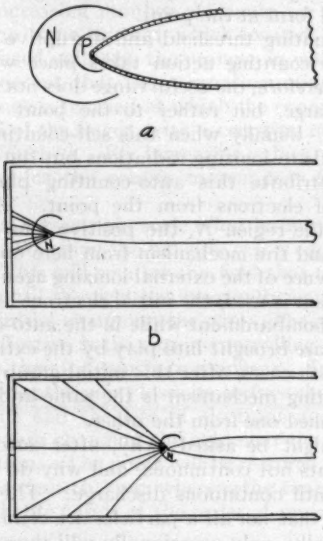

FIG. 5. (*a*) Schematic diagram of point showing insulating layer and regions P, in which the positive ions acquire sufficient energy to eject electrons when they strike the point, and N, in which the electrons acquire sufficient energy in their free flights to ionize the gas. (*b*) Exaggerated diagram showing region N when point is near the front of the counter tube and (*c*) when it is moved farther back.

effective in ejecting electrons. Furthermore, region N becomes larger and the process will continue until a larger charge has accumulated on the insulating layer. Likewise, region P becomes larger and the probability of registering counts increases. To bring about the condition where the positive ions again become ineffective in ejecting electrons, the charge on the insulating layer must become greater (to reduce the field in region P) as the voltage on the counter increases. This increase in voltage, however, cannot go on indefinitely; it can only increase to the

point where one of two things may happen, either one producing the same result. The charge on the insulating layer may become so great that the layer is disrupted, or the positive ions may have sufficient energy to penetrate and dissipate the layer. It might be mentioned here that Joffe [17] found that thin layers (below 0.1 μ) of a dielectric could withstand fields 500 times as great as thick layers. When the layer is disrupted the discharge will be continuous even at a reduced voltage. By reducing the voltage sufficiently the discharge will cease and a new insulating layer will form at the point.

Between the counting threshold and disruptive discharge a region exists in which the counting action takes place without an external ionizing agent. Therefore, the useful range does not extend to the point of disruptive discharge, but rather to the point where the counter "counts by itself." Usually when this self-counting takes place, the counter also responds to ionizing radiations but the two cannot be distinguished. We attribute this auto-counting phenomenon to cold cathode emission of electrons from the point. These auto-emission electrons ionize in the region N, the positive ions thus formed move towards the point and the mechanism from here on is the same as for counting in the presence of the external ionizing agent. In other words, when an ionizing agent is used, the initial electrons leaving the point are due to positive ion bombardment while in the auto-counting case these necessary electrons are brought into play by the extremely high field at the point, but in both cases, after this initial group of electrons leaves the point, the counting mechanism is the same and therefore the two cannot be distinguished one from the other.

The question might be asked: why, after auto-electron emission occurs, are the counts not continuous, and why do they increase with increased voltage until continuous discharge? The answer might be: for the same reason that not all α particles are counted at threshold of counting. Statistically, only occasionally will there be a count. The counts increase as the ion-producing conditions become more efficient or as the probability increases. However, a better answer would be the Schroteffekt of auto-electronic emission, due to the granular nature of electricity. We know of no experiments where this effect has been observed directly, but the same theoretical considerations should hold in auto-electronic emission as are applied to thermionic emission.

OBSERVATIONS FAVORABLE TO AND EVIDENCE SUBSTANTIATING THE PROPOSED THEORY.

1. The Lag Phenomenon.

The lag phenomenon found by Zeleny [5] and observed by the authors necessitates a high resistance layer at the point as postulated by Zeleny and later by Geiger.[4] This well-known phenomenon has been amply described in the literature and needs no further discussion here.

The writers have also experimented with the counter of Fig. 1 in which a bakelite cap similar in form to the brass one was substituted for the latter. While the counter was functioning in the presence of the ray emitter it was possible to stop the action by placing a finger on the bakelite cap, and upon removal of the finger some time elapsed before the counts began to reappear. At any given value of (x) this time for recovery decreases rapidly as the tube voltage increases.* For a given voltage the reappearance of the counts is characterized by a single count, then a gradually increasing number of counts per unit time until the counting rate is the same as that which existed before the cap was touched. At increased voltages, the manner in which the counts reappear is the same, except that the normal counting rate occurs much sooner, until a voltage is reached at which the counts reappear immediately upon removal of the finger, with a counting rate the same as that before the cap was touched. For increasing values of (x) (in the positive direction) the time for the counts to reappear for a given voltage decreases.

This shows on a megascopic scale what happens at the point on a microscopic scale. Touching the bakelite cap is equivalent to changing the charge on it. This will so alter the field that the counting mechanism cannot start until the field has been sufficiently restored by reaccumulation of the initial charge on the cap. The closer the point is to the cap the more effective is the cap in controlling the field and, therefore, the longer the time for counts to reappear. In a like manner, the high resistance layer at the point loses its surface charge due to the addition of positive ions and the loss of electrons and is charged in the opposite direction, thus changing the field near the point sufficiently to stop further ionization. The point will be restored to its initial state in a time probably less than 10^{-3} sec. whereas the time for reappearance of counts with the bakelite cap is of the order of several seconds.

2. Dependence of Counter Action on Ionizing Rays.

That the counter responds to α-rays at a voltage lower than that at which it responds to β-rays, and to β-rays at a voltage lower than that for γ-rays, yet all three respond at the same upper limit and with about the same effect, has been observed by many experimenters. Such discrimination toward the ionizing particle can be explained on a statistical basis. In order that the counting action may take place, the number of positive ions initially produced within the effective region of the counter must produce electrons which in turn must produce at least as many ions as were originally formed by the ionizing agent. If less

* Since the time for the counts to reappear is of the order of a few seconds, it is best to choose the lowest voltage of a series of such observations as that which produces a fairly continuous counting rate so that the time between counts is not comparable to the time for reappearance of counts.

than the original number of ions is produced by the outgoing electrons, the counter action cannot take place. At the lowest voltage at which the counter responds to α-rays, only a small part of these rays is counted, because the building up mechanism is of a statistical nature and, therefore, for the same number of ions formed initially, a count may sometimes be registered and sometimes not. As the voltage is increased, regions N and P increase, the ionizing processes become more efficient, and the probability of registering a count becomes greater for the same number of initial ions produced. Also, at this higher voltage a much smaller number of initial ions may now produce a count. At this increased voltage nearly all of the strongly ionizing rays (α) are counted, yet only a few of the less strongly ionizing rays (β) are counted. This explains the three separate thresholds for the counting of α-, β-, and γ-rays.

3. Dependence of Counter Action on Direction of Ionizing Rays.

The experiments of Kutzner,[18] of Bothe [19] and of others show the dependence upon direction at which the ionizing radiation enters the chamber. Their experimental results show that there is an effective region inside the chamber in which the rays are most likely to produce a count. Briefly, the longest path the ionizing agent can have in this effective region is most conducive to producing a count. This simply means that the more initial ions present in the effective region the greater the probability that a count will be instituted. The reasoning follows that of the preceding paragraph.

4. The Existence of Three Characteristic Curves *A*, *B*, *C*, Fig. 2.

The origin of the three distinct curves A, B, C has been given in the section on results. It will be noted that the ratio of curve C to curve A is essentially constant. The ratio of curve B to curve A, however, varies considerably as the point approaches the front of the counter, thus making the working range much larger when the point has been moved back into the counter cylinder. For an explanation of this fact consider the two exaggerated diagrams (b) and (c) of Fig. 5. For the same potential gradient at the point, region N will be greater in diagram (b) than in diagram (c). Therefore, any electron ejected from the point in diagram (b) will have a greater chance of initiating the counter action and thus produce a count. For the same fields at the points in diagrams (b) and (c), the auto-electrons may produce a count in (b) but not in (c) even though the number of auto-electrons given off is exactly the same in both cases. In case (b) the auto-electrons will produce a number of ions which, for the sake of explanation only, is equal to the ions produced by an α-ray. In diagram (c) the ions produced by the same number of auto-electrons will be equivalent, let us say, to those produced by a β-ray. As explained in the preceding sections, the counter will be sensitive to α-rays and not to β-rays under certain conditions. Thus the

auto-counting will take place in (*b*) and not in (*c*). In order to produce auto counts in (*c*) the voltage must be increased. This simply amounts to saying that the larger the number of ions produced by the initial auto-electrons, the greater the probability of producing a count.

By replacing the flat cap on the counter by a hemispherical one, when the point is near the front of the counter, the ratio of curve *B* to curve *A* is increased and therefore the working range is increased. This again follows the same reasoning, for with the hemispherical cap, the gradient drops more rapidly at distances outward from the point than it would with the flat cap. Region *N*, for the same field at the point, will be smaller for the hemispherical cap than for the flat one and the range for the hemispherical cap is larger as observed experimentally by the authors.

The fact that a point and plane combination works better when the plane has a hole towards which the point is directed than when the hole is not present is again explained on the same basis, since for any given position of the point, region *N* is smaller when the hole is present in the plane than when not present, for the same field at the point in both cases. Using the plane with hole and now moving the point away from the plane, the working range becomes greater for the same reasons. The point and plane combination cannot have a larger working range than the point and cylinder (other factors not varied) because in the former the region *N* must always be larger than with the conventional counter for the same gradient at the point.

5. Dependence of Counter Action on Gas and Cathode Material.

Kolhörster [20] shows that the working range in hydrogen is about twice as great as in air. Curve *A* is determined to a large extent by the mean free path of the positive ion in the gas. In air the mean free path of the positive ion is less than in hydrogen; therefore, the voltage for the threshold in air should be higher. However, the numerical value of these results has little significance, for not only is the gas different but also the points, insofar as their surfaces are concerned, are different in the two cases, and we might reasonably expect that different energy positive ions would be required to eject electrons from the different films that surround the point in the different gases. Curves *B* coincide very nearly for air and hydrogen. This curve is due to auto-electron emission and should be very nearly the same for the two gases.

Holst and Oosterhuis [21] have shown parallelism between sparking potential for different cathode materials and Richardson's constant Φ, and that the minimum sparking potential [18] may vary as much as 1 to 3 with different cathode materials, and as 1 to 3 with different gases. These results are not at variance with our theory.

Fig. 4 shows the relation between the pressure and voltage at the threshold of counting and is similar to the ones obtained by Kolhörster.[20]

At pressures near atmospheric the relation is nearly linear but for low pressures the slope is much steeper. As the mean free path becomes smaller, the voltage required to give sufficient energy to the ions in their free flights must increase at a smaller rate than the pressure, because the gradient decreases as we move away from the point. The smaller the regions P and N become, the more nearly linear must be the relationship between pressure and voltage for the threshold of counting.

6. Cold Cathode Emission.

The only initiating mechanism, which is necessary and sufficient to produce counts, appears to be the production of electrons from the point. Once the point is in a state, conditioned or otherwise, in which it can emit electrons, the counter will function. For instance, it has been amply demonstrated that photoelectrons ejected from a Pt point will produce counts.[23] Now, when the ionizing radiation is used, it is assumed that electrons are ejected from the point by positive ion bombardment during the counting process. But in dealing with the self-counting curve B, one must look for a different origin of these necessary initiating electrons. It cannot be stray ions because curve B sometimes lies below curve A, and in that case strong rays fail to register counts, yet the auto-counting continues. It would be too much to assume that the stray ionization would be stronger than from a strong β-ray source. We are thus led to the only other possible mechanism for the ejection of electrons from the point, namely, cold cathode emission. These electrons thus ejected would pass out through the strongest field and produce positive ions which would likewise travel through this field, giving the greatest probability of starting the counting mechanism.

DeBoer [24] says fields of 10^5 to 10^6 volts per cm are strong enough to give appreciable cold cathode emission. Flowers [25] and Ahearn [26] have found cold cathode emission at half a million volts per cm. Millikan and Lauritsen [27] have found that for strong fields the logarithm of the field current is a linear function of the reciprocal of the field strength, which indicates that small currents should exist for fields below those at which measurements on field currents are usually made. Emeléus calculated a field of 7.5×10^5 volts per cm at a spherical point of 0.02 mm radius in one of his counters. By making use of the equation given by Compton and Langmuir [28] we calculate for the onset of auto-counting, for a point and plane combination, a field of 5×10^5 volts per cm. An estimate of the radius of curvature of the point was made by using a photomicrograph of the point. In view of all this it appears that the field at the point is great enough to produce cold cathode emission.

CONCLUSION.

Frankly, we do not know exactly what the condition of the point must be; whether it must have a catalytic surface as suggested by

Curtiss [7] or not still remains an open question. We do not know that no special treatment of the point to produce a catalytic surface is required, but this does not preclude the possibility of the point's already having a catalytic surface. In the words of Zeleny,[29] "There is at present no unanimity of opinion as to how a point counter functions nor as to what gives a surface the property essential for a reliable counter." Moisture will always cause the counter to cease functioning, while xylene vapor may start it. The most important thing about a point appears to be its shape. It should be fairly sharp but not too sharp. A point may be made to function by applying nitric acid; yet with further application of nitric acid it will again cease to function. The explanation is that the first application of the acid removed burrs and also changed the surface film. With the second application, the point is again made rough by an uneven action of the acid. This was observed many times. The indication is that for the same kind of surface film the action still depends upon the contour of the point. Dunning and Skinner [30] have developed a method for making tungsten-ball points which are fairly satisfactory over a long period of time. The ball, formed under surface tension, acquires a smooth, spherical contour.

That space charge,[8] increased pressure due to electrical wind,[6] ionization by photoelectric effect, and series resistance all contribute to the operation of the counter in some degree, we do not question. However, none of these nor any combination can explain all the experimental results obtained by us and by others. Space charge, for example, should work just as well for a very sharp point as for a blunter one—so should increased pressure due to electrical wind—but this is not the case. One of the clinching arguments used by Emeléus in his excellent work is that the counter ceases to function at very low pressures, that is, there is no interruption once the ionizing process starts. Likewise, using this same phenomenon, we may argue that at very low pressures the insulating layer, if present at all, will not reform after it is once disrupted and therefore there is nothing to stop the ionizing process, and the counter goes into a continuous discharge. Similarly, at very sharp points the insulating layer cannot form properly.

We conclude that (1) Zeleny's gas layer, (2) ejection of electrons from the point by positive ion bombardment and (3) auto-electronic emission, are required to account for the complete action of the counter. It is obviously true that the largest working range for a Geiger Point Counter depends upon the geometry of the counter itself, upon the gas used, and upon the shape and material of the point. This does not mean that a counter of almost any geometry or almost any electrode material cannot be made, but rather that there is a most suitable geometry of a counter which gives the best relationship between the regions N and P, and the field at the point for good operation.

The experimental results were obtained at Temple University and

the writers wish to express their appreciation to Prof. C. S. McGinnis for his helpful suggestions during the investigation and to Mr. H. H. Kramer for his assistance in the mechanical construction of the apparatus. The helpful advice and criticism of Edith L. Morgan are gratefully acknowledged.

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ON THE REACTION OF FLUIDS AND FLUID JETS.

BY

RUPEN EKSERGIAN, Ph.D.,

Edward G. Budd Manufacturing Company, Philadelphia, Pennsylvania.

The object of this paper is to present some simple applications on fluid flow problems, with special emphasis on methods of analysis for the reactions on constraining channels both stationary and moving. Also, consideration will be given to the energy relations involved in such fluid systems. The entire subject is of particular interest in connection with jet propulsion of airplanes, the rocket projectile and in the dynamical relations with moving blade systems with expanding fluids.

The problem becomes complicated when the density of the fluid is not constant. In this case, we must also consider the thermodynamic relations in the flow. Furthermore, in many problems the flow rate itself varies with time. The solution for unsteady flow offers great difficulties. However, in many cases steady flow conditions can be approximated and it becomes important to appraise the degree of approximation.

ELEMENTARY CHARACTERISTICS OF JET PROPULSION.

Some early applications of jet propulsion were concerned in the hydraulic propulsion of ships, in which water was sucked in at the forward end. After passing through suitable pumps the water was ejected through nozzles at the rear.

Let:

u = The absolute velocity of the ship (ft./sec.).
v = The relative velocity of ejection of the fluid at the exit section of the nozzle (ft./sec.).
a = Exit area of jet (sq. ft.).
$G = \rho a v$ = Flow rate (lbs. of fluid per sec.).
Φ = Torque exerted by circulating pump (lb. ft.).
ω = Ang. velocity of pump impeller.
ϵ_h = Hydraulic efficiency.

We now consider the entire guide system consisting of entrance guide channels, pump impeller and exit channel with nozzle, as acting on the fluid. Let X be the fore and aft component of the reaction between the guide system and fluid. The final absolute velocity is $v - u$ (ft./sec.) to the rear. On the other hand, the absolute entrance velocity is assumed nil. Then, the overall equation of momentum gives:

$$X = \frac{G}{g}(v - u) = \frac{\rho a v}{g}(v - u) \text{ (lbs.)} \qquad (1)$$

for the axial reaction of the turbine and guide system on the fluid to the rear. By the principle of action and reaction, it also equals the reaction of the fluid on the guide system, or ship, in a forward direction.

Next, we consider the overall energy relations of the fluid passing through the guide system. The work done by the pump is $\Phi\omega$, while the work done by the guide system reacting on the flow is $-Xu$. The net work is equated to the change in kinetic energy of the fluid:

$$\epsilon_h \Phi\omega - Xu = \frac{1}{2}\frac{G}{g}(v-u)^2 \tag{2}$$

or,

$$\Phi\omega - Xu = \frac{1}{2}\frac{G}{g}(v-u)^2 + \frac{G}{2g}\epsilon v^2. \tag{2'}$$

Combining (1) and (2), we have:

$$\epsilon_h \Phi\omega = \frac{G}{g}(v-u)^2 + \frac{G}{g}(v-u)u,$$

$$\therefore \quad \epsilon_h \Phi\omega = \frac{G}{2g}(v^2 - u^2), \tag{3}$$

which shows the work done by the hydraulic pumping system, times the hydraulic efficiency, always equals the change in the relative kinetic energy of the exit and entrance flow to the pump.

We define the efficiency of propulsion, as:

$$\epsilon_P = \frac{Xu}{\epsilon_h \Phi\omega} = \frac{2(v-u)u}{v^2 - u^2} = \frac{2u}{v+u} \tag{4}$$

while the overall efficiency is:

$$\epsilon = \epsilon_h \cdot \epsilon_P = \frac{Xu}{\Phi\omega} = \frac{2\epsilon_h \cdot u}{v+u}. \tag{5}$$

The unsuccessful attempts of jet ship propulsion has been due to inherent low values of ϵ_h.

Expression (4) shows that for good propulsion efficiency, v, the relative exit velocity of the fluid at the jet should be in the same order as the speed of the ship u. To accomplish this practically, and to utilize large power output, equation (3) shows that G or the fluid moved must be large.

Propulsion Power and Thrust Variation.

The thrust due to the change in linear momentum of the jet is:

$$X = \frac{\rho a v^2}{g}\left(1 - \frac{u}{v}\right) \tag{6}$$

and the corresponding propulsion power is:

$$Xu = \frac{\rho a v^2}{g}\left(1 - \frac{u}{v}\right)\frac{u}{v}. \tag{7}$$

We have seen that for maximum efficiency u approaches v. To obtain a finite thrust, the mass flow $\rho av/g$ must be very large. Thus, the diameter of the exit area of the orifice which determines a must be large.

The expression for thrust power for constant flow rate is a parabolic function of the speed of the ship. With constant flow rate the maximum power would then occur when $u = v/2$, that is, when the speed of the ship reaches half the speed of the jet. In this case, the maximum efficiency cannot exceed $2/3$. Actually, the flow rate itself varies according to equation (3). As u approaches v the velocity of the jet, the mass flow increases. The thrust X therefore, does not fall off necessarily as a linear function with the speed ratio u/c.

For this reason, with a given power input, the starting thrust is in the order or less than twice the thrust at rated speed. Since the speed of the ship or plane must be necessarily high for good efficiency, we see the starting thrust is inherently low, without additional power input during the starting period.

The hydraulic jet propulsion really simplifies, with vast improvement to a simple propeller propulsion, where the free intake approaching the propeller reduces hydraulic losses. Also, large masses can be handled by a relatively slow and large diameter propeller.

REACTION OF A FLUID ON A MOVING BLADE SYSTEM.

Propeller theory has been developed from two angles. First, the propeller is considered as an actuator causing a change in linear momentum in a slip stream. In the inflow to the suction side at pressure p_1, the slip stream narrows down with an increase in relative velocity from v to v_P, where v_P is the mean velocity of the fluid through the propeller. The pressure is increased to p_2 in crossing the propeller. The slip stream continues to the outer boundary corresponding to the common pressure of the surroundings, p_0, but with an increased velocity v_m.

The propeller thrust, T, is obviously:

$$T = \rho A_P v_P (v_m - v) = (p_2 - p_1) A_P, \tag{8}$$

where A_P = effective area of slip stream through propeller. But, by Bernoulli's Theorem:

$$p_0 + \frac{\rho v^2}{2g} = p_1 + \frac{\rho v_P^2}{2g} \quad \text{for the approach portion,}$$

$$p_0 + \frac{\rho v_m^2}{2g} = p_2 + \frac{\rho v_P^2}{2g} \quad \text{for the leaving portion,}$$

so that:

$$p_2 - p_1 = \frac{\rho}{2g}(v_m^2 - v^2) = \frac{\rho}{2g}(v_m + v)(v_m - v), \tag{9}$$

and from (8) we note the remarkable relationship, namely that:

$$v_P = \tfrac{1}{2}(v_m + v).$$

Following the reasoning in establishing equation (3), we also have, for the power input:

$$\epsilon_h \Phi\omega = \frac{G}{2g}\left[v_m^2 - v^2\right], \tag{10}$$

where $G = \rho A_P v_P$; therefore:

$$\epsilon_h \Phi\omega = \tfrac{1}{2}T(v_m + v). \tag{11}$$

Now, since the initial intake velocity relative to the moving propeller system is v, this also is the absolute velocity u of the propeller system forward. Therefore, the work of propulsion is Tu.

Then, the efficiency, since $u = v$, is:

$$\epsilon = \frac{Tv}{\Phi\omega} = \frac{2\epsilon_h u}{u + v_m},$$

which is identical with equation (5). Obviously, the momentum and energy relationships are the same for either type of propulsion system. On the other hand, for slow or medium speeds, ϵ_h is much higher for propeller propulsion. Also, the diameter of the propeller can be made large, so that a great mass flow in the slip stream is possible.

The theory given above is known as the "Rankine" or "Froude" theory of propulsion. It hardly is acceptable for propeller design. The theory in no way shows the nature of the torque reaction between the propeller and the slip stream. It indicates no details for proportioning only a few bladed propeller.

FLUID REACTION ON BLADE SYSTEM.

The mechanism for noting the nature of the reaction for producing both torque and thrust can be shown by considering a single row of blades, Plate I.

Figure (1) shows the slip stream with entrance relative velocity v_1, and exit relative velocity v_2. The relative velocity across the blades is v_P.

Figure (2) shows the blade system which moves with a tangential velocity u due to the angular velocity ω; i.e. $v = \omega r$.

The fluid exerts a tangential reaction component X and a normal component Y on the blade system. Let F be the normal cross-section for the flow across the blades. Let p_0 equal the pressure of the en-

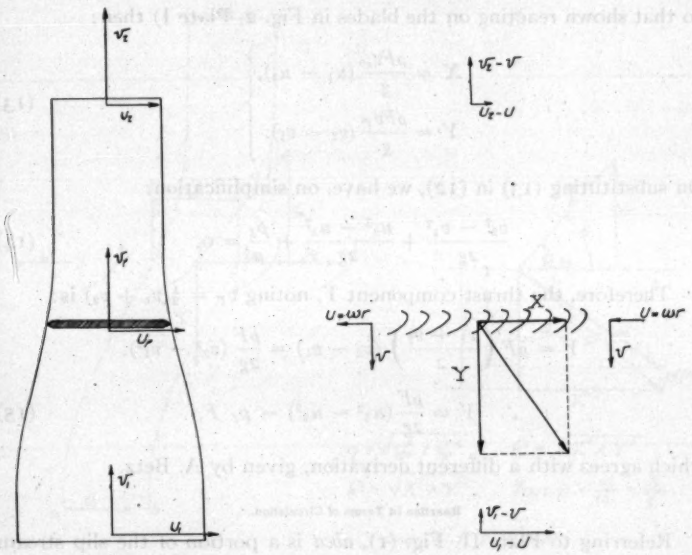


PLATE I

vironment or surrounding medium corresponding to the terminal sections of the slip stream. The absolute entrance and exit velocity components are $v_1 - v$, $u_1 - u$ and $v_2 - v$, $u_2 - u$ respectively.

We will express the friction loss in terms of a pressure head p_f. Then, the energy equation for the slip stream in crossing the moving blades is:

$$Xu - Yv = \rho F v_p \left\{ \left[\frac{p_0}{\rho} + \frac{(v_2 - v)^2 + (u_2 - u)^2}{2g} \right] - \left[\frac{p_0}{\rho} + \frac{(v_1 - v)^2 + (u_1 - u)^2}{2g} \right] + \frac{p_f}{\rho} \right\}. \quad (12)$$

Considering the change in momentum of the fluid due to the reactions X and Y of the blades (which act on the fluid in opposite directions

to that shown reacting on the blades in Fig. 2, Plate I) then:

$$\left.\begin{aligned} X &= \frac{\rho F v_P}{g}(u_1 - u_2), \\ Y &= \frac{\rho F v_P}{g}(v_2 - v_1). \end{aligned}\right\} \tag{13}$$

On substituting (13) in (12), we have, on simplification:

$$\frac{v_2^2 - v_1^2}{2g} + \frac{u_2^2 - u_1^2}{2g} + \frac{p_f}{\rho} = 0. \tag{14}$$

Therefore, the thrust component Y, noting $v_P = \frac{1}{2}(v_1 + v_2)$ is:

$$Y = \rho F\left(\frac{v_1 + v_2}{2}\right)(v_2 - v_1) = \frac{\rho F}{2g}(v_2^2 - v_1^2),$$

$$\therefore \quad Y = \frac{\rho F}{2g}(u_1^2 - u_2^2) - p_f \cdot F, \tag{15}$$

which agrees with a different derivation, given by A. Betz.

Reaction In Terms of Circulation.

Referring to Plate II, Fig. (1), *abcd* is a portion of the slip stream passing through a single blade element of pitch δ and depth h, so that $F = \Sigma\delta h$. The isolated portion is a stream tube bounded by stream lines *ad* and *bc* respectively with boundary sections *ab* and *dc* at the intake and outlet of the slip stream where the pressure reduces to that of the environment p_0.

The lateral relative velocity along *ab* is u_1 and along *dc* is u_2. We define the circulation Γ as the line integral of the velocity around the boundary of the stream tube. Since the line integral *bc* and *da* cancel each other, the net line integral is:

$$\Gamma = (u_1 - u_2)\delta \quad \text{for the circulation}$$

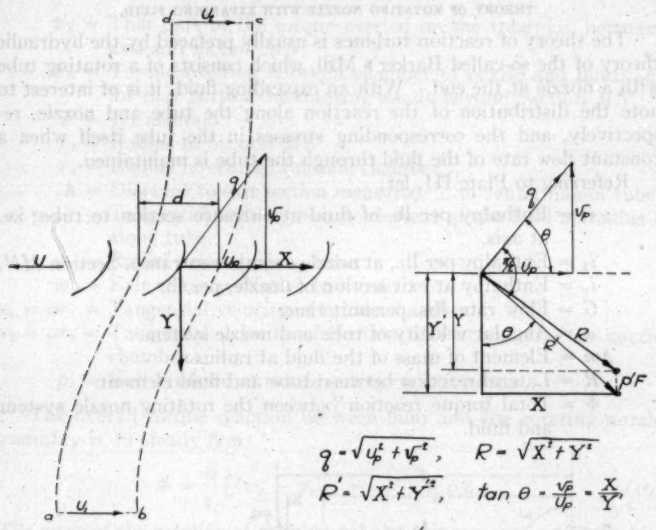
Noting $F = h\Sigma\delta$ the reaction on the blades is:

$$X = h\Sigma\frac{\rho}{g} \cdot v_P(u_1 - u_2)\delta = h\Sigma\frac{\rho}{g}\Gamma \cdot v_P, \tag{16}$$

$$Y = h\Sigma\frac{\rho}{g} \cdot u_P(u_1 - u_2)\delta - p_f F$$

$$= h\Sigma\frac{\rho}{g}\Gamma u_P - h p_f \Sigma\delta, \tag{17}$$

where $v_P = \frac{1}{2}(v_1 + v_2)$ and $u_P = \frac{1}{2}(u_1 + u_2)$.



$$q = \sqrt{u_p^2 + v_p^2}, \qquad R = \sqrt{X^2 + Y^2}$$

$$R' = \sqrt{X^2 + Y'^2}, \qquad \tan\theta = \frac{v_p}{u_p} = \frac{X}{Y},$$

$$\text{and } Y' = Y + p'F$$

PLATE II

Referring to Fig. 2, Plate II

$$\tan\theta = \frac{v_P}{u_P} = \frac{X}{Y'} = \frac{X}{Y + hp_f\Sigma\delta},$$

$$q = \sqrt{{v_P}^2 + {u_P}^2}, \qquad R = \sqrt{X^2 + Y^2}, \qquad R' = \sqrt{X^2 + Y'^2},$$

where

$$Y' = \Sigma h \frac{\rho}{g} \Gamma u_P.$$

Hence it can be concluded that R' is perpendicular to q and

$$R' = h\Sigma \frac{\rho}{g} \Gamma q \qquad (18)$$

which is the value obtained for the lift on an aerofoil according to the Kutta Joukowski theory. The reactions on the blading and the velocities through the blading are shown in the vector diagram Fig. 2, Plate II.

THEORY OF ROTATING NOZZLE WITH EXPANDING FLUID.

The theory of reaction turbines is usually prefaced by the hydraulic theory of the so-called Barker's Mill, which consists of a rotating tube with a nozzle at the end. With an expanding fluid, it is of interest to note the distribution of the reaction along the tube and nozzle, respectively, and the corresponding stresses in the tube itself when a constant flow rate of the fluid through the tube is maintained.

Referring to Plate III, let:

i_1 = Enthalpy per lb. of fluid at entrance section to tube; i.e. at axis.
i_2 = Enthalpy per lb., at nozzle chamber entrance, Section MN.
i_e = Enthalpy at exit section of nozzle, per lb.
G = Flow rate, lbs. per unit time.
ω = Angular velocity of tube and nozzle system.
Δm = Element of mass of the fluid at radius r.
R = Lateral reaction between tube and fluid element.
Φ = Total torque reaction between the rotating nozzle system and fluid.

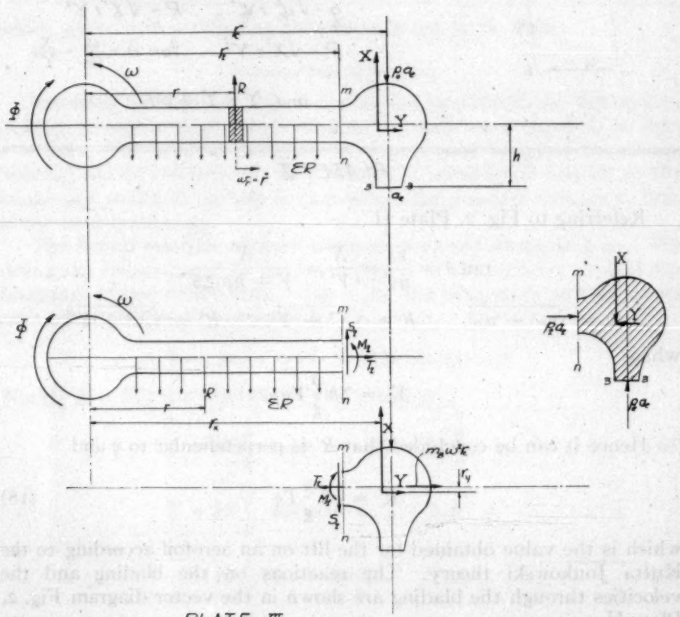


PLATE III

Φ_A = That part of the torque exerted on the tube; i.e. between axis and section MN.
Φ_B = Torque reaction between chamber of nozzle and fluid; i.e. for that part of the rotating nozzle beyond Section MN.
P = Jet reaction.
r_2 = Radius to Section MN.
r_c = Radius to center of nozzle chamber.
h = Distance to exit section measured $\perp$ to center line of tube.
$u = \omega r$ = Tangential velocity of any element of the fluid at radius r along tube.
w_r = Radial velocity along tube.
w_e = Exit velocity relative to nozzle.
$u_c = \omega r_c$ = Tangential velocity at nozzle chamber.
$u_2 = \omega r_2$ = Tangential velocity of Section MN at entrance to nozzle chamber.
p_e = Exit pressure at nozzle acting on exit area a_e.

The overall torque reaction between fluid and the rotating nozzle assembly is, in steady flow:

$$\Phi = \frac{G}{g}\left[(w_e - u_c)r_c - \omega h^2\right] + p_e a_e r_c. \tag{19}$$

The work of the reaction of the channel and exit pressure *on the fluid* is:

$$-\Phi\omega + p_e a_e r_c \cdot \omega.$$

Therefore the equation of energy per lb. of fluid is:

$$i_1 - i_e - A\left[(w_e - u_c)r_c - \omega h^2\right]\frac{\omega}{g} = \frac{A(w_e - u_c)^2}{2g}. \tag{20}$$

From which we obtain, noting $u_c = \omega r_c$:

$$i_1 - i_e + A\,\frac{\omega^2(r^2 + h^2)}{2g} = A\,\frac{w_e^2}{2g} \tag{21}$$

where the last term on the left side corresponds to the work of the centrifugal forces acting on the fluid along the tube and nozzle extension.

Let us now separate the tube and chamber by an imaginary Section MN (Fig. 2). Considering the reaction along the tube alone, we have, for the motion of any element:

$$Rr = \frac{d}{dt}\left[\Delta m r^2\omega\right] = 2\Delta m r\,\frac{dr}{dt}\cdot\omega,$$

$$\therefore \quad R = 2\Delta m \dot{r}\omega, \qquad \text{where} \qquad \dot{r} = \frac{dr}{dt}.$$

The work done by the "Coriolis" reaction, R, is:

$$R\omega r = 2dm\dot{r}\omega^2 r = 2\,\frac{dm}{dt}\,\omega^2 r dr,$$

where $dm/dt = G/g$ = flow rate. The total work done by the reaction forces on the tube portion is:

$$\Sigma R\omega r = 2\frac{G}{g}\omega^2\int_0^{r_2} r^2 dr = \frac{G}{g}\omega^2 r_2^2 = \frac{G}{g}u_2^2. \tag{22}$$

Otherwise, we note the angular momentum of the fluid generated per unit time across Section MN is $G/g \times u_2 r_2$. Since the entrance angular momentum to the tube is nil, the torque reaction between the fluid and the tube proper (not including nozzle) is:

$$\Phi_A = -\frac{G}{g}u_2 r_2 \qquad \text{and} \qquad -\Phi_A\omega = \frac{G}{g}u_2^2, \tag{22'}$$

where obviously $\Phi_A = -\Sigma Rr$. The energy equation of the flow to Section MN at the end of the tube, per lb. of fluid, is:

$$i_1 - i_2 + A\frac{u_2^2}{g} = A\left[\frac{u_2^2 + w_{r_2}^2}{2g}\right].$$

So that:

$$i_1 - i_2 + A\frac{u_2^2}{2g} = A\frac{w_{r_2}^2}{2g}, \tag{23}$$

where:

$$A/g\int \omega^2 r dr = \frac{Au_2^2}{g} = \text{Centrifugal work along tube.}$$

Considering the fluid flowing through the chamber alone, i.e., beyond section MN to the nozzle exit section, the entrance angular momentum per unit time across Section MN is $-G/g \times u_2 r_2$ and the exit angular momentum is: $G/g[(w_e - u_c)r_c - \omega h^2]$, where h is the distance to exit section measured perpendicular to the center line of tube. Therefore, the torque, acting on the portion of the housing above Section MN, is:

$$\Phi_B = \frac{G}{g}[(w_e - u_c)r_c - \omega h^2 + u_2 r_2] + p_e a_e r_c. \tag{24}$$

If h is small compared with r_c and, assuming and $r_2 = r_c$, then neglecting p_e:

$$\Phi_B = Pr_c = \frac{G}{g}w_e r_c \qquad \therefore \qquad P = \frac{G}{g}w_e \qquad (approx.). \tag{25}$$

The energy equation for the fluid between Section MN and the exit section of the nozzle is:

$$i_1 - i_e - \frac{A}{g}\Phi_B\omega + p_e a_e r_c\omega$$
$$= \frac{A}{2g}\{[(w_e - u_c)^2 + \omega^2 h^2] - [u_2^2 + w_{r_2}^2]\}, \tag{26}$$

and this reduces to:

$$i_2 - i_e + A\frac{u_c^2 - u_2^2}{2g} + A\frac{\omega^2 h^2}{2g} = \frac{w_e^2 - w_{r2}^2}{2g}. \qquad (27)$$

That is, the change in enthalpy, plus the work of the centrifugal forces between the entrance and exit sections to the nozzle itself, equals the change in relative kinetic energy.

Obviously $\Phi = \Phi_A + \Phi_B$ and $i_1 - i_e = (i_1 - i_2) + (i_2 - i_e)$ as a check on equations (19), (22) and (24), and (21), (25) and (27) respectively.

It is important to note, however, that the resultant torque exerted by the rotating channel on its shaft differs from Φ by the unbalanced pressure of the environment due to the opening a_e at the nozzle. Therefore, the resultant torque on the shaft is:

$$\Phi_R = \Phi - p_0 a_e r_c \qquad (p_0 = \text{Pressure of Environment}).$$

$$\therefore \quad \Phi_R = \frac{G}{g}\left[(w_e - u_c)r_c - \omega h^2\right] + (p_e - p_0)a_e r_c. \qquad (28)$$

But:

$$(p_e - p_0)a_e = \frac{G}{g}(w_0 - w_e),$$

$$\therefore \quad \Phi_R = \frac{G}{g}\left[(w_0 - u_c)r_c - \omega h^2\right], \qquad (29)$$

where w_0 is the maximum velocity of the c. of g. of the flow in expanding down to the environment pressure p_0.

Calculation of Stresses.*

To calculate the stresses between the tube and nozzle chamber at Section MN, it is also necessary to obtain the component translation reactions on the nozzle and chamber; i.e. that portion of the rotating channels above Section MN.

The change in radial momentum per unit time of the fluid flow

* The moment of the reaction of the fluid about the shaft axis acting on the nozzle chamber between Section MN and the exit section 33, is Φ_B; where Φ_B is due to the change of the angular momentum of the fluid between these sections. Therefore, $\Phi_B = Xr_x - Yr_y$ where X and Y are the components of the resultant reaction of the fluid on the nozzle chamber, and r_x and r_y are their respective coordinates relative to the axis of rotation. We also note X and Y are due to the changes in the translation momentums of the fluid through the nozzle chamber along the tangential and radial directions.

Therefore, the moment of the fluid reaction, with the unbalanced external pressure of the environment, about the centroid of Section MN is:

$$M_2 = X(r_x - r_2) - Yr_y - p_0 a_e(r_e - r_2),$$
$$M_2 = \Phi_B - (X - p_0 a_e)r_2 - p_0 a_e r_c,$$

where $X - p_0 a_e$ is the shear force at Section MN.

between the entrance Section MN and the exit section of the nozzle is due to the radial pressure acting across Section MN, minus the reaction of the nozzle chamber which is assumed to act radially toward the center.

$$\therefore \quad p_2 a_2 - Y = \frac{G}{g}[\omega h - w_{r_2}],$$

$$\therefore \quad Y = \frac{G}{g}[w_{r_2} - \omega h] + p_2 a_2, \tag{30}$$

radially outward on the nozzle chamber. In like manner, for the tangential component:

$$X = \frac{G}{g}[(w_e - u_c) + \omega r_2] + p_e a_e. \tag{31}$$

We have already calculated the moment of the reaction of the fluid on the isolated nozzle chamber $= \Phi_B$.

Consider the nozzle and its chamber; i.e., that part of the rotating channel above Section MN. Let T_2 = the total tension, S_2 = total shear, and M_2 = the bending moment at Section MN. Let m_B = the mass of the nozzle chamber above Section MN with its c. of g. assumed at r_c. Then:

$$\left.\begin{aligned} T_2 &= Y + m_B \omega^2 r_c, \\ S_2 &= X - p_0 a_e. \end{aligned}\right\} \tag{32}$$

To calculate M_2 we take moments of all the reactions acting on m_B including the reactions across Section MN above the shaft axis. Referring to Fig. 2, we first note that the moment of the reactions of the fluid on m_B above the shaft axis is already included in the torque Φ_B. The moment of the reaction of the tube across Section MN about the shaft axis is $-(M_2 + S_2 r_2)$. Finally, we must include the moment of the unbalanced pressure of the environment p_0 acting on the projected area a_e.

$$\therefore \quad \Phi_B - M_2 - S_2 r_2 - p_0 a_e r_c = 0,$$

since T_2 has no moment about the axis. Therefore:

$$M_2 = \Phi_B - (X - p_0 a_e) r_2 - p_0 a_e r_c,$$

for the bending moment across Section MN. The resultant stress components at Section MN are:

$$T_2 = \frac{G}{g}[w_{r_2} - \omega h] + p_2 a_2 + m_B \omega^2 r_c, \tag{33}$$

$$S_2 = \frac{G}{g}[w_e - u_c + \omega r_2] + (p_e - p_0) a_e = \frac{G}{g}[w_0 - u_c + \omega r_2], \tag{34}$$

$$M_2 = \frac{G}{g}[(w_e - u_c)(r_c - r_2) - \omega h^2] + (p_e - p_0)(r_c - r_2)a_e$$

$$= \frac{G}{g}[(w_0 - u_c)(r_c - r_2) - \omega h^2]. \quad (35)$$

Let us now isolate the tube below Section MN. The reactions on the tube consist of the distributed fluid reactions along the tube with moment Φ_A about the shaft axis, and the reaction of the nozzle mass m_B across Section MN and, finally, the balancing torque reaction Φ_R at the shaft axis. Therefore, taking moments about the shaft axis for all the forces acting on the tube, we have:

$$S_2 r_2 + M_2 + \Phi_A = \Phi_R,$$
$$\therefore \quad \Phi_B - p_0 a_e r_c + \Phi_A = \Phi_R.$$

This agrees with the overall value of Φ_R previously calculated.

FLUID REACTION ON NOZZLES.

Let us first consider the simple hydraulic case of the reaction of a steady flowing liquid through a conical nozzle. Consider the fluid flow between terminal Sections 1 and 2 of the nozzle with pressures p_1 and p_2 at these sections. Let F_1 and F_2 be the corresponding areas, and F_x, the cross section at distance x along the nozzle from Section 1. If the angle of obliquity of the walls is α, then:

$$F_x = \pi r^2, \qquad \text{where} \qquad r = r_1 - x \tan \alpha.$$

To calculate the axial thrust X of the liquid against the walls of the tube, we have:

$$X = \int_{F_2}^{F_1} p \frac{dA}{\sin \alpha} \cdot \sin \alpha = \int_{F_2}^{F_1} p dA,$$

where, by Bernoulli's theorem:

$$p = p_1 + \frac{\rho c_1^2}{2g} - \frac{\rho c_x^2}{2g},$$

where

$$\rho_x F_x c_x = \rho F_1 c_1.$$

Therefore:

$$X = \int_{F_2}^{F_1} \left(p_1 + \frac{c_1^2}{2g}\right) dF_x - \int_{F_2}^{F_1} \frac{\rho F_1^2 c_1^2}{2g} \cdot \frac{dF_x}{F_x^2},$$

$$\therefore \quad X = \left(p_1 + \frac{\rho c_1^2}{2g}\right)(F_1 - F_2) + \frac{\rho F_1^2 c_1^2}{2g}\left(\frac{F_2 - F_1}{F_1 F_2}\right), \quad (36)$$

which gives the axial thrust of the fluid against the wall of the nozzle

in terms of the pressure and velocity at the entrance section. Noting that:

$$p_1 = p_2 + \frac{\rho}{2g}(c_2^2 - c_1^2) \qquad \text{and} \qquad F_1c_1 = F_2c_2.$$

On substituting these values in Equation (36), we have:

$$X = p_1F_1 - p_2F_2 + \frac{\rho F_1c_1}{g}(c_1 - c_2),$$

or

$$p_1F_1 - p_2F_2 - X = \frac{G}{g}(c_2 - c_1), \tag{37}$$

where

$$G = \rho F_1c_1 = \rho F_2c_2 = \rho F_xc_x, \tag{38}$$

which is recognized as the momentum equation. To express (37) in the form (36), we must also use *Bernoulli's Equation* and the *continuity equation* (38).

The reaction X acts forward on the nozzle and causes a tension in the pipe at Section F_1 unless otherwise supported. If, on the other hand, the nozzle is attached to a containing vessel or a right angle elbow which feeds the fluid into the nozzle, a backward net pressure p_1F_1 acts on the container itself, so that the net reaction due to the fluid on the assembly including the nozzle is:

$$R = p_1F_1 - X = \frac{G}{g}(c_2 - c_1) + p_2F_2.$$

That is:

$$R - p_2F_2 = \frac{G}{g}(c_2 - c_1), \tag{39}$$

where R is the overall reaction of the fluid on the complete assembly and acts to the rear; i.e., opposite to the motion of the jet. Equation (39) is also recognized as the overall momentum equation.

Reaction of an Expanding Fluid on Nozzle.

We have two cases (1) with the nozzle stationary and (2) with the nozzle assembly as part of a moving frame which has a translatory velocity u.

With a moving frame system, let us consider the fluid between the entrance and exit section of the nozzle. Let X be the axial component of the reaction of the nozzle walls on the fluid between Sections 1 and 2. Let the terminal pressures be p_1 and p_2 acting on terminal cross sections F_1 and F_2 of the fluid passing through the nozzle. Let i_1 and i_2 be the corresponding enthalpies per pound of fluid crossing these sections. The absolute terminal velocities are c_1 and c_2.

Then, the following relations govern an adiabatic flow assumed at constant rate G lbs/sec. across any section.

$$G(i_1 - i_2) + Xu + (p_2F_2 - p_1F_1)u = \frac{AG}{2g}(c_2^2 - c_1^2),$$

and

$$p_1F_1 - p_2F_2 - X = \frac{G}{g}(c_2 - c_1),$$

which are the overall energy and momentum equations respectively. The work done by the reaction of the walls on the fluid is Xu. Since i is the enthalpy per pound transferred across any section, it is associated with the relative displacement of the flow. Since the terminal sections themselves move with the common velocity of the frame, u, additional work is done by the end pressures acting on the fluid system between the terminal sections.

Let the terminal relative velocities be: $w_2 = c_2 - u$ and $w_1 = c_1 - u$, then on combining the previous equations we have:

$$i_1 - i_2 = \frac{A}{2g}(c_2^2 - c_1^2 + 2(c_2 - c_1)u),$$

$$\therefore \quad i_1 - i_2 = \frac{A}{2g}(w_2^2 - w_1^2) \qquad \text{for a moving nozzle.}$$

For this reason, the energy equation may be stated either in terms of absolute velocities for fixed nozzles or in terms of relative velocities when the nozzle and its frame is subjected to a translatory motion in the axial direction.

In the following, we will consider flow with stationary nozzles. The overall energy equation for an adiabatic expansion with steady flow for any section F_x is,

$$A\left[\frac{c_x^2 - c_1^2}{2g}\right] = i_1 - i_x \qquad \text{(per lb.)}, \tag{40}$$

where i_x = the enthalpy at Section F_x. For an ideal gas:

$$i_1 - i_x = \frac{k}{k-1}(p_1v_1 - p_xv_x), \tag{41}$$

where $k = c_P/c_v$ = ratio of the specific heats.

For a converging nozzle, let F_m be the throat area at critical pressure p_m. Then the velocity at this section, corresponds to the accoustic velocity c_a where:

$$c_a = \sqrt{gkpv}.$$

Therefore: neglecting c_1

$$c_m = \sqrt{2g\frac{k}{k-1}(p_1v_1 - p_mv_m)} = \sqrt{gkp_mv_m},$$

from which we find:

$$p_mv_m = \frac{2}{k+1}p_1v_1,$$

$$\therefore \quad c_m = \sqrt{2g\frac{k}{k+1}p_1v_1} = \text{velocity at throat.}$$

The area at the throat section must be consistent with the equation of continuity, that is:

$$G = \rho_mF_mc_m = \rho_xF_xc_x,$$

where the subscript x corresponds to any section F_x.

For an adiabatic expansion:

$$p_1v_1^k = p_mv_m^k, \qquad \text{with} \qquad p_mv_m = \frac{2}{k+1}p_1v_1,$$

$$\therefore \quad \left(\frac{v_1}{v_m}\right)^k = \frac{2}{k+1}\cdot\frac{v_1}{v_m}.$$

Then:

$$\frac{v_1}{v_m} = \left(\frac{2}{k+1}\right)^{1/(k-1)} \qquad \text{and} \qquad p_m = \left(\frac{2}{k+1}\right)^{k/(k-1)}p_1.$$

The density at the throat section is:

$$\rho_m = \frac{1}{v_m} = \frac{1}{v_1}\left(\frac{2}{k+1}\right)^{1/(k-1)}.$$

The flow rate is $B\rho_mF_mc_m = G$ where B is a coefficient to allow for a reduction in velocity due to friction. Hence:

$$G = B\left(\frac{2}{k+1}\right)^{1/(k-1)}F_m\sqrt{\frac{2gk}{k+1}\cdot\frac{p_1}{v_1}}, \tag{42}$$

which measures the gas flow rate in terms of the throat section of the nozzle and the entrance pressure and specific volume.

The velocity at the *exit section* F_2 is:

$$c_2 = B\sqrt{2g\frac{k}{k-1}(p_1v_1 - p_2v_2)}.$$

Noting

$$p_2v_2^k = p_1v_1^k, \qquad \therefore \quad v_2 = \left(\frac{p_1}{p_2}\right)^{1/k}v_1,$$

$$\therefore \quad c_2 = B\sqrt{2g\frac{k}{k-1}\cdot p_1v_1\left[1 - \left(\frac{p_2}{p_1}\right)^{(k-1)/k}\right]}. \tag{43}$$

Reaction on Nozzle.

Let ΣX, as before, be the axial reaction between the fluid and nozzle casing. Then, for the time rate of change of the momentum between entrance and exit sections, we have:

$$p_1F_1 - p_2F_2 + \Sigma X = \frac{G}{g}(c_2 - c_1). \tag{44}$$

If there is to be no net reaction on the casing, then $\Sigma X = 0$. If, on the other hand, we consider sections between entrance and exit for only the converging conical part of the nozzle, then ΣX must inherently be negative, since the pressure must be positive and the axial projection of the surface is against the flow.

Since c_2 is usually large compared with c_1, in an ordinary jet expansion, the entrance velocity can be frequently neglected. Then:

$$R = p_1F_1 + \Sigma X = \frac{G}{g}c_2 + p_2F_2,$$

where R is defined as the total reaction of the jet. When the nozzle assembly includes both the chamber and nozzle in an integral casing, then ΣX *includes* p_1F_1, so that:

$$R = \Sigma X = X_1 - X_2 = \frac{G}{g}c_2 + p_2F_2.$$

On substituting (42) and (43) in $(G/g)c_2$, we have:

$$R - p_2F_2 = 2B^2 \cdot F_m \frac{k}{\sqrt{k^2 - 1}} \left(\frac{2}{k+1}\right)^{1/(k-1)} \cdot p_1 \sqrt{1 - \left(\frac{p_2}{p_1}\right)^{(k-1)/k}}. \tag{45}$$

This equation is remarkable in that the properties of the fluid do not appear other than the terminal pressures.

ROCKET PROPULSION.

The reaction of the jet in this case is due to the rate of momentum generated by the burning of the powder mass. Let μ = the total powder charge, and r, the per cent. of powder burned per unit time. Then, the rate of burning of the powder is $r\mu$. If the velocity of ejection of the powder gases, which is also the relative velocity of the gases at exit, is v, then the reaction of the jet is:

$$R = r\mu v \qquad \text{where} \qquad \frac{G}{g} = r\mu. \tag{46}$$

The mass of the rocket including the remaining powder charge at any time t is $M_0 - r\mu t$ where M_0 is the total initial mass including the powder charge.

Assuming the rocket to be fired vertically with acceleration $\ddot{y}$, then:

$$M\ddot{y} = R - Mg, \qquad \text{where} \qquad M = M_0 - r\mu t,$$

$$\therefore \quad \ddot{y} = \frac{R}{M} - g = \frac{r\mu v}{M_0 - r\mu t} - g. \tag{47}$$

On integrating for the velocity we have:

$$\dot{y} = -v \log_e (M_0 - r\mu t) - gt + c,$$

when:

$$t = 0, \qquad \dot{y} = 0, \qquad \therefore \quad C = v \log_e M_0,$$

$$\therefore \quad \dot{y} = v \log_e \frac{M_0}{M} - gt, \qquad (M = M_0 - r\mu t),$$

for the velocity at any time t.

For the maximum velocity $\ddot{y} = 0$, then:

$$r\mu v - M_0 g + gr\mu t = 0,$$

$$\therefore \quad gt = \frac{M_0 g - r\mu v}{r\mu} = \frac{M_0 - M}{r\mu} \cdot g,$$

since at maximum velocity $r\mu v = Mg$.

Hence the maximum velocity reached is:

$$\dot{y}_{max} = v \log_e \frac{M_0}{M} - \frac{g}{r\mu} (M_0 - M), \tag{48}$$

where

$$\left| \begin{array}{l} M = M_0 - r\mu t, \\ t = \dfrac{M_0 g - r\mu v}{gr\mu}. \end{array} \right.$$

In the above analysis, we have neglected air resistance. We have also assumed a constant rate of burning of the powder which may vary considerably. The above relations, therefore, can only be considered as extreme values.

Efficiency of Rocket Propulsion.

The energy of the powder E is connected with the energy of the jet by the relation $\frac{1}{2}\mu v^2 = \epsilon_b E$, which assumes a constant rate of burning during the total time t_0 for the consumption of the powder. Since the percent of powder burned per unit time is r and the rate of burning is:

$$\frac{d\mu}{dt} = r\mu = \frac{\mu}{t_0}, \qquad \therefore \quad t_0 = \frac{1}{r}.$$

The jet reaction as before is $R = r\mu v$, where we assume the powder and oxygen for combustion is self contained within the rocket itself.

If now, we fire the rocket in a horizontal direction, gravity is normal to the direction of motion and does no work. We also neglect air resistance. In this hypothetical ideal case, the work done by the reaction of the jet is:

$$W_j = \int_0^{t_0} R \cdot u dt, \qquad \text{where} \quad \left| \begin{array}{l} u = \int_0^t \frac{R}{M} dt, \\ M = M_0 - r\mu t, \end{array} \right.$$

for the complete burning of the powder. Then we define the propulsion efficiency as ϵ_P:

$$\epsilon_P = \frac{W_j}{\epsilon_b E}, \qquad \therefore \quad \epsilon = \frac{W_j}{E},$$

where $\epsilon = \epsilon_P \cdot \epsilon_b$ is the rocket efficiency which corresponds to the ratio of the maximum work of the reaction of the jet to the energy of the powder.

Now

$$u = \int \frac{r\mu v dt}{M_0 - r\mu t} = -v \log_e (M_0 - r\mu t) + C,$$

when

$$t = 0, \qquad C = v \log_e M_0,$$

$$\therefore \quad u = v[\log_e M_0 - \log_e (M_0 - r\mu t)] = v \log_e \frac{M_0}{M}.$$

Therefore, the work of propulsion is:

$$W_j = r\mu v^2 \int_0^{t_0} [\log_e M_0 - \log_e (M_0 - r\mu t)] dt.$$

Since:

$$\log_e (M_0 - r\mu t) = \log_e M_0 - \frac{r\mu t}{M_0} - \frac{r^2\mu^2 t^2}{2M_0^2} - \frac{r^3\mu^3 t^3}{3M_0^3} + \cdots,$$

$$\therefore \quad W_j = \left[\frac{r\mu t_0^2}{2M_0} + \frac{r^2\mu^2 t_0^3}{6M_0^2} + \frac{r^3\mu^3 t_0^4}{12M_0^3} + \cdots \right] r\mu v^2.$$

Since $t_0 = 1/r$ assuming constant burning rate, the maximum efficiency is (under assumed conditions):

$$\epsilon = \frac{\epsilon_b W_j}{\frac{1}{2}\mu v^2} = \left[\frac{\mu}{M_0} + \frac{\mu^2}{3M_0^2} + \frac{\mu^3}{6M_0^3} + \cdots \right] \epsilon_b. \tag{49}$$

That is, the efficiency of rocket propulsion is in the order of the ratio of the powder charge used for propulsion to the initial total mass of the rocket. The actual efficiency may be considerably less, due to the effect of gravity component and air resistance reducing the tangential velocity of the rocket u, and also to variations of the reaction R due to considerable variation in the rate of the burning of the powder itself.

It is also evident that so far as the maximum utilization per lb. of propulsive powder is concerned, that rocket propulsion is ineffective compared with the firing of a projectile from a gun, where the thermal efficiency is in the direct order of ϵ_b.

JET PROPULSION OF AIRPLANES.

In the case of jet propulsion, as a motive power unit for high speed airplanes, the jet reaction is a function of the speed of the airplane itself. A possible supply of continuous energy is by means of a gas turbine-compressor installation, in which the intake air is compressed through a compressor and then passes through a combustion chamber. Either a part or all the combustion gases pass through the gas turbine which is proportioned to only drive the compressor. In the former, we have a parallel flow. In the latter or series flow, there is a pressure drop through the turbine, and then the gases pass directly to the exit nozzle. Due to mixing losses, in the subsequent mixing of the gases in parallel flow, only the continuous series flow system will be considered where the entire flow of combustion gases pass through the turbine and then to the exit nozzle.

Let v = exit relative velocity of the gases, and u = the velocity of the airplane. We further assume the initial absolute velocity of the intake air used for combustion to be nil. Otherwise, it has a relative velocity u with respect to the airplane. The fuel itself is stored in the moving airplane and has an initial absolute velocity u, i.e. the same as that of the airplane itself. Let G_A and G_f be the weight flow rate of the intake air and fuel consumed, respectively. Then the combustion gases have a flow rate $G = G_A + G_f$.

Now, irrespective of the distribution of friction forces, moving blade reactions of the compressor and turbine, or reactions of the walls of the flow channels, *the total net reaction exerted by the entire power plant and jet assembly on the fluid and combustion products*, is:

$$R = \frac{G_A}{g}(v - u) + \frac{G_f}{g} \cdot v,$$

since the total *change* in velocity for the intake gases is $v - u$ and for the fuel burned is v. Since $G = G_A + G_f$ and G_f is small compared with G_A.

$$\therefore \quad R = \frac{G}{g}(v - u) \qquad \text{(approx.)}. \tag{50}$$

Therefore, by the principle of action and reaction, R is likewise the reaction of the fluid on the power plant and airplane forward.

The propulsion *power* is therefore:

$$P_p = \frac{G}{g}(v - u)u = \frac{G}{g}v^2\left(1 - \frac{u}{v}\right)\frac{u}{v},$$

where u/v is the speed ratio, i.e. the ratio of velocity of the airplane to the exit relative jet velocity. With constant flow rate, G, the propulsion power varies as a parabolic function of the speed ratio u/v. Moreover, the maximum output occurs when $u/v = \frac{1}{2}$. This equation also shows that with moderate speed planes, the jet velocity v is necessarily low, so that for large output, the mass flow rate G must be large. For this reason jet propulsion is best adapted for very high speed planes.

Let us now consider the energy relation for the complete circuit, that is, for the fluid of mass flow rate G/g entering the power unit, with intake relative velocity u and leaving with exit relative velocity v. This assumes the intake absolute velocity is nil and the exit absolute velocity is $v - u$. Let Φ = the torque of the compressor blades on the fluid flow through the compressor. Since the turbine and compressor are mechanically connected, the torque reaction of the turbine blades on the fluid must also be Φ. The energy transfer between the moving blade systems and fluid for either turbine or compressor is $\Phi\omega$, where ω is the common angular velocity of the connecting shaft. We note, however, $\Phi\omega$ is positive for the compressor and negative for the turbine. Without fluid losses therefore, the turbine-compressor unit "floats" on the circuit and does not enter into the energy relation. However, if we take the fluid losses into consideration the situation is entirely different. To account for fluid friction and shock losses through the compressor, the net input from the compressor to the fluid is $\epsilon_c\Phi\omega$, that is, the input is always less than $\Phi\omega$. To account for similar losses through the driving turbine, evidently the total loss of energy must exceed $\Phi\omega$. If ϵ_t accounts for the turbine losses, then the net *input* from the turbine to the fluid, is $-\Phi\omega/\epsilon_t$. That is, energy is taken from the circuit, per unit time, by this amount.

In a thermodynamic circuit, as the case is actually, the losses do not show up explicitly, the change in enthalpy across either the turbine or compressor being the same, i.e. $\Phi\omega/G$ per unit mass, irrespective of the losses. The properties of the fluid at the terminal sections, however, are markedly changed by the losses, resulting in an increase of entropy. The increase of entropy accounts for a loss of availability for doing mechanical work, ultimately resulting in a decrease of the kinetic energy of the exit jet used for propulsion. Moreover, the effect of the losses are more severe in a hydraulic circuit than in a thermodynamic circuit due to the beneficial reheat which is most effective when the losses occur at the higher temperatures. For this reason, in the preceding statements, the friction coefficients may be regarded as efficiency factors with respect to an ideal frictionless adiabatic flow.

Exactly as in the energy transfer across the blades of a turbine or compressor, we have a similar energy transfer between the fluid and the power unit, such as walls, blades, etc., which exert pressures on the fluid. This energy transfer is measured by the work per unit time on

the power plant Ru. The corresponding reaction work per unit time on the fluid, is therefore $-Ru$. In this case the energy transfer is of a translatory nature.

When the gases, discharged from the compressor, pass through the combustion chamber, they absorb the energy of combustion Q_1 for G pounds of fluid. On the other hand, the heat contents in the exhaust gases in the jet is Q_2' for G pounds of fluid in an ideal reversible flow with no losses. With an isothermal compression, Q_2' includes the heat abstracted during compression. Therefore, the net heat energy available in the fluid flow is $\Delta E = A(Q_1 - Q_2')$ per unit time. This is the basic available energy rate for propulsion. The actual heat contents in the exhaust gases due to the actual irreversible cycle is Q_2 where $Q_2 > Q_2'$.

The change in kinetic energy is $\frac{1}{2}G/g(c_2^2 - c_1^2)$ where $c_2 = v - u$ and $c_1 = 0$.

Therefore, the energy equation for the fluid passing through the power unit is:

$$\epsilon_h \left[\epsilon_c \Phi\omega + \Delta E - \frac{1}{\epsilon_t} \Phi\omega \right] - Ru = \frac{G}{2g} (v - u)^2,^* \qquad (51)$$

where ϵ_h accounts for losses in the channels.

From the momentum equation, however, we have:

$$R = \frac{G}{g} (v - u).$$

Therefore, on substituting R in equation (51), we have:

$$\epsilon_h \left[\Delta E - \left(\frac{1 - \epsilon_c \epsilon_t}{\epsilon_t} \right) \Phi\omega \right] = \frac{G}{2g} (v^2 - u^2). \qquad (52)$$

With no losses, $\epsilon_c = \epsilon_t = 1$, so that the second term completely disappears, then:

$$\Delta E = \frac{G}{2g} (v^2 - u^2).$$

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* From a thermodynamic aspect, the losses do not appear explicitly. Therefore, since the energy transfer at the compressor and turbine are equal and opposite, the *actual* energy is:

$$A(Q_1 - Q_2) - Ru = \frac{G}{2g} (v - u)^2.$$

On combining with equation (51) where $\Delta E = A(Q_1 - Q_2')$ for the *available* energy, we have:

$$A(Q_2 - \epsilon_h Q_2') = \epsilon_h \left(\frac{1 - \epsilon_c \epsilon_t}{\epsilon_t} \right) \Phi\omega + (1 - \epsilon_h) Q_1$$

which gives an expression for the increase of heat energy in the exhaust gases over that corresponding to an ideal reversible expansion.

With losses, assuming $\epsilon_c = 0.6$ and $\epsilon_t = 0.7$, then:

$$\epsilon_h[\Delta E - 0.83\Phi\omega] = \frac{G}{2g}(v^2 - u^2).$$

This shows for energy to be transferred to the jet $\Delta E > 0.83\Phi\omega$, otherwise no output is possible. The ratio $\Delta E/\Phi\omega$ is a very important ratio and is directly coupled with efficiencies anticipated for turbine and compressor.

Further, only a portion of the kinetic energy generated in the fluid $(G/2g)(v^2 - u^2)$ is available for propulsion. We define the propulsion efficiency, relative to the kinetic energy generated in the flow stream as:

$$\epsilon_P = \frac{Ru}{(G/2g)(v^2 - u^2)} = \frac{2u}{v + u}.$$

The propulsion efficiency in terms of the speed ratio u/v is:

$$\epsilon_P = \frac{2 \cdot u/v}{1 + u/v}.$$

Thus, assuming constant flow rate, the efficiency at maximum propulsion output is $\epsilon_P = \frac{2}{3}$. Since $Ru = \epsilon_P \cdot (G/2g)(v^2 - u^2)$, on substituting in the energy equation, we have:

$$\therefore \quad Ru = \epsilon_h \epsilon_p \left[\Delta E - \left(\frac{1 - \epsilon_c \epsilon_t}{\epsilon_t} \right) \Phi\omega \right]. \tag{53}$$

If $\Phi\omega/\Delta E = \gamma$, then, the over-all efficiency is:

$$\epsilon = \frac{\epsilon_h \cdot \epsilon_p}{\epsilon_t} [\epsilon_t - \gamma(1 - \epsilon_c \epsilon_t)]. \tag{54}$$

For this reason γ should be as small as possible, or in other words the ratio of the available energy of combustion to the work of compression should be as large as possible.

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APPENDIX I.

Mixing of Fluids and Ejectors.

Let us consider the mixing of two fluids with flow rates G_1 and G_2, where the mixture has a flow rate $G = G_1 + G_2$. Let ΔF_1 and ΔF_2 be the corresponding entrance sections and F the exit section for the mixture. In the case of a cylindrical mixing chamber, $F = \Delta F_1 + \Delta F_2$. Let $\Sigma\Delta X$ be the axial reaction of the walls on the fluid in the mixing chamber between the above terminal sections. Then, since the action and reaction between the parts of the fluid in the mixing have no effect on the overall momentum:

$$p_1\Delta F_1 + p_2\Delta F_2 - pF + \Sigma\Delta X = \frac{G}{g}c - \left(\frac{G_1}{g}c_1 + \frac{G_2}{g}c_2 \right),$$

where c = velocity at exit and c_1 and c_2 are entrance velocities of driving and entrained fluids G_1 and G_2. When the mixing chamber can be approximated to a common pressure, then

$$p(\Delta F_1 + \Delta F_2) - pF + \Sigma \Delta X = 0;$$
$$\therefore \quad Gc = G_1c_1 + G_2c_2,$$

with
$$G = G_1 + G_2.$$

The overall energy equation is:

$$\frac{1}{A}[G_1(i_1 - i) - G_2(i_2 - i)] = G_1\left(\frac{c^2 - c_1^2}{2g}\right) + G_2\left(\frac{c^2 - c_2^2}{2g}\right).$$

On combining both equations:

$$G_1(i - i_1) + G_2(i - i_2) = \frac{A}{2}\frac{G_1G_2}{G_1 + G_2}(c_1 - c_2)^2, \qquad \left(A = \frac{1}{778}\right),$$

from which the enthalpy of the mixture i can be calculated in terms of the entrance quantities i_1 and i_2. In obtaining the properties of the fluid at the exit section by means of a temperature entropy diagram we note the change in enthalpies occur *at constant pressure, approximately.*

On the other hand, if the pressure also builds up as with a cylindrical mixing chamber, the momentum equation for the mixing of the fluids is (noting $\Sigma \Delta X = 0$):

$$\frac{G}{g}c - \left(\frac{G_1}{g}c_1 + \frac{G_2}{g}c_2\right) = F(p_i - p);$$
$$\therefore \quad gF(p - p_i) + Gc = H,$$

where H/g is the *initial momentum*, and p_i is the common initial pressure at entrance.

The overall energy equation remains the same. If expressed in terms of temperature, i.e. $i = c_PT$, then:

$$G\left(c_PT + A\frac{c^2}{2g}\right) = G_1\left(c_PT_1 + A\frac{c_1^2}{2g}\right) + G_2\left(c_PT_2 + A\frac{c_2^2}{2g}\right);$$
$$\therefore \quad G\left(c_PT + A\frac{c^2}{2g}\right) = E,$$

where E is the *initial energy*.

Assuming a simple equation of state $pV = BT$, then $V = \frac{Fc}{G}$ at the terminal section at temperature T;

$$\therefore \quad pFc = GBT.$$

The above three relations permit a solution:

$$A\frac{G}{2g}c^3 + \frac{c_PF}{B}\left[p_i + \frac{H - Gc}{F \cdot g}\right]c = E;$$
$$\therefore \quad G\left[\frac{c_P}{Bg} - \frac{A}{2g}\right]c^3 - \frac{c_PF}{B}\left[p_i + \frac{H}{Fg}\right]c + E = 0,$$

which is a quadratic equation in terms of the terminal velocity c. Knowing c we compute T from the energy equation, and then p from the equation of state.

The above analysis, at best, can only be considered as a very rough approximation of the actual conditions of mixing for the following reasons:

First, we do not know, except by experiment, the length of the mixing process, and therefore the proper terminal section to be assumed. Second, the flow across the various sections considered are by no means uniform, so that the proper mean velocity, as well as the mean pressure at a section, is difficult to appraise. Third, the reaction of the walls and the pressure distribution in the mixing chamber can only be approximated as shown above. Fourth, the properties of the fluid across a section vary, so that the terminal conditions for the energy equation are at best approximate.

Any mixing process results in a large loss of available energy. The increase of unavailable energy is:

$$T_0\Sigma\Delta\phi = T_0\{G_1(\phi - \phi_1) + G_2(\phi - \phi_2)\},$$

where:

$$\phi - \phi_1 = c_P \log_e \frac{T}{T_1} - AB \log_e \frac{p}{p_1},$$

$$\phi - \phi_2 = c_P \log_e \frac{T}{T_2} - AB \log_e \frac{p}{p_2},$$

T_0 = temp. of environment.

In the case of constant pressure mixing the last terms are omitted, and the mixing temperature T is obtained from the expression:

$$G_1c_P(T - T_1) + G_2c_P(T - T_2) = \frac{A}{2} \cdot \frac{G_1G_2}{G_1 + G_2}(c_1 - c_2)^2.$$

APPENDIX II.

Change of Section and Pressure Drop Along the Converging-Diverging Nozzle.

The significance of the critical and throat pressure in the expansion of a fluid through a nozzle and its connection with the accoustic velocity is best shown through the differential equation of the flow through a nozzle.

If we differentiate the equation of continuity, $Fc = Gv$, where F is any cross-section, G = the flow rate and v = the specific volume, and the mean velocity across the section is c, then:

$$\frac{dc}{c} + \frac{dF}{F} = \frac{dv}{v}. \tag{a}$$

Assuming an adiabatic expansion $pv^k = c$, then:

$$v^k dp + kpv^{k-1} \cdot dv = 0, \qquad \therefore \quad \frac{dv}{v} = -\frac{dp}{kp}.$$

$$\therefore \quad \frac{dc}{c} + \frac{dF}{F} + \frac{dp}{kp} = 0. \tag{b}$$

For the equation of motion of any element along the flow:

$$\frac{cdc}{g} = -vdp. \tag{c}$$

On substituting in (b), we have:

$$\left(\frac{gv}{c^2} - \frac{1}{kp}\right) dp = \frac{dF}{F};$$

$$\therefore \quad dp = -\frac{dF}{F} \cdot \frac{kpc^2}{c^2 - gkpv}. \tag{d}$$

Now the acoustic velocity $c_a = \sqrt{gkpv}$ so that the differential equation of flow through the nozzle in terms of the acoustic velocity is:

$$\frac{1}{p} \cdot \frac{dp}{dx} = -\frac{kc^2}{c^2 - c_a^2} \cdot \frac{1}{F}\frac{dF}{dx}. \tag{e}$$

To obtain a drop in pressure gradient throughout the nozzle, the right hand side must be always negative. In the converging part $c < c_a$, so that dF/dx must be negative, that is, the cross-section reduces on increase of x.

At the minimum section of the nozzle, i.e. at the throat, $c = c_a$ = the acoustic velocity. Since we have a *finite* pressure drop, therefore $dF/dx = 0$. In the subsequent flow in the diverging portion dF/dx is positive, while c exceeds the acoustic velocity.

Let us now consider a converging-diverging nozzle in which c never exceeds the acoustic velocity c_a. There is a drop of pressure in the converging portion as before. In the diverging

portion, however, the second term becomes positive, thus indicating a rise in pressure as in a venturi tube.

When the exit pressure ratio, p_e/p_1, is reduced by lowering the back pressure at exit, the pressure at the throat becomes equal to the critical pressure p_m, and the velocity through the throat area equals c_a, the velocity of sound. Further lowering of the back pressure in no way affects the ratio p_m/p_1, and the flow rate becomes fixed equal to $G = e_m F_m c_a$. The exit velocity, however, increases so that the momentum of the jet increases with decreased back pressures.

Fluid Shock.

When the back pressure is higher than the design exit pressure, but not so high as to prevent full expansion to the critical pressure p_m at the throat, shock losses occur beyond the throat in the diverging part of the nozzle. The pressure in the nozzle where the shock loss starts is then considerably less than the raised exit back pressure. A detachment of the flow from the walls also takes place in this low pressure region, accompanied by a sudden rise in pressure due to shock loss.

Shock losses result in a lowering of the exit velocity and therefore the propulsion efficiency of the jet. For this reason the divergent part of the nozzle should be small in the order of 10°–12°, thus reducing the tendency of detachment of the jet, particularly for possible varying back pressures.

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NOTES FROM THE NATIONAL BUREAU OF STANDARDS.*

DEVELOPMENT OF TURBIDIMETRIC STANDARDS AND TESTS OF TURBIDIMETERS FOR OIL-IN-WATER EMULSIONS.

The steam condensates from the reciprocating engines of small aircraft carriers and transports contain some of the lubricating oil in the form of an emulsion. This oil must be removed by filters before the water can be used again in the ship's boilers. Consequently, turbidimeters and turbidity standards are needed for determining the amount of emulsified oil in the steam condensates and clarified filtrates.

At the request of the Research Section of the Navy Bureau of Ships, George G. Manov, N. J. DeLollis, and S. F. Acree, of the National Bureau of Standards, undertook in May, 1943, the development and production of standards of turbidity for oil-in-water emulsions ranging from 0 to 2 parts per million oil, that would deteriorate as little as possible during a 6-month period. It was further requested that an effort be made to stimulate the development and production of shipboard turbidimeters by manufacturers, and that the resulting instruments be tested at the Bureau for compliance with performance specifications set up jointly by the Bureau of Ships and the Bureau of Standards.

Six commercial and laboratory type turbidimeters were tested as to suitability for use on board rolling, vibrating ships under the conditions of high temperatures and humidities usually present in the engine rooms. Turbidimeters with photocells and galvanometer or "magic-eye" balance detectors were studied and calibrated with various turbidity standards. In the most successful instrument two opposing photocells are used to measure the difference in light transmission of air and of turbidity standards or of a cuvette containing the test sample. A "magic-eye" tube serves to detect the balance point. The meter is rugged, portable, and plugs into any 110-v. a.c. line for operation. It is insensitive to tilting, vibration, and shock, and may be calibrated readily by any of the types of standards developed at the Bureau. The readings of the instrument are reproducible to $\pm$ 0.05 p.p.m. oil and the steadiness appears to be quite satisfactory. The calibration of the scale, which can be graduated to read directly in p.p.m., is linear and does not shift with time. Likewise, a humidity of 100 per cent. does not affect the operation of the electrical circuits of the turbidimeter.

Three types of turbidity standards that can be turned out by mass

* Communicated by the Director.

production methods, have been developed. The first consists of films of gelatin in which colloidal carbon is dispersed uniformly in amounts giving decreased transmittancies corresponding to 0.6, 1.6, 1.7, and 2.3 p.p.m. oil. These films are cemented with Canada balsam between borosilicate glass sheets.

In the second type, rectangular blocks of crown borosilicate glass, with four 25 × 40 mm. faces A, B, C, and D, were polished to give graded light transmittancies. In the standardization, incident light strikes face B, passes through the 40 mm. length of glass, through face A, and then to a photocell. Face A is more highly polished than face B, and the combination gives about 91 per cent. light transmission—equivalent to that of distilled water (0.0 p.p.m. oil) in a rectangular glass cuvette with the same light path. By rotating the block 180° so that face B is nearest the photocell, the calibration corresponds approximately to 0.3 p.p.m. of emulsified oil in the same cuvette. Faces C and D are slightly rougher than faces A and B. The calibrations of the four faces, as above, are linear and correspond to approximately 0.0, 0.3, 0.7, and 1.5 p.p.m. oil in the cuvettes in two types of turbidimeters. These glass standards were produced in the Bureau's optical shop with satisfactory tolerances.

The third type of turbidity standard consists of colloidal carbon (India ink) in a 0.5 per cent. solution of Aerosol as dispersing agent together with 0.1 per cent. orthochlorophenol as a preservative, and 0.002 molal phosphate buffer (pH 7). Accelerated aging tests at 140° F. showed that these carbon standards are reasonably stable, whereas the oil emulsion standards used for calibrations change slowly and must be made up again at intervals.

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DOPES FOR AUTOMOBILE FUELS.

Shortage of gasoline combined, perhaps, with the large amount of money in the hands of many motorists, has led to a great increase in the activities of purveyors of fuel "dopes," reputed to increase fuel mileage and otherwise improve the operation of automobiles.

Several of these dopes have been widely advertised, even to the extent of radio programs. Although the Bureau has tested hundreds of them in the past, both in the laboratory and in vehicles on the road, without finding beneficial results in any case, extensive tests are being completed on some of the newcomers in the field to make doubly sure of the conclusions. The main conclusions as they now stand are that none of the fuel dopes used in small amounts, such as an ounce or so per gallon of gasoline, have any measurable effect, desirable or otherwise, on the operation of the automobile or the miles per gallon of fuel.

ANALYTICAL DETERMINATION OF AROMATIC HYDROCARBONS BY ADSORPTION.

In a paper under the foregoing title, which appears in the April number of the *Journal of Research* (RP1582), B. J. Mair and A. F. Forziati of the American Petroleum Institute Research Project 6 describe a simple method for determining the amount of aromatic hydrocarbons in a mixture of hydrocarbons, as in the gasoline fraction of petroleum. The mixture to be analyzed is filtered through a column of solid adsorbent. An aromatic-free filtrate is obtained containing the paraffin, naphthene, or olefin hydrocarbon which was associated in the original solution with the quantity of aromatic hydrocarbon which has been absorbed. The concentration of aromatic hydrocarbon in an unknown solution is determined by means of calibration curves, established from experiments on known solutions, which show the amount of aromatic-free filtrate produced by the standard absorbent from solutions of various concentrations of the aromatic hydrocarbon.

Results of experiments are given for several concentrations of eight binary solutions of an aromatic hydrocarbon with a paraffin or naphthene hydrocarbon and for three concentrations of a solution consisting of an aromatic hydrocarbon with a paraffin and an olefin. These experiments show that, if the temperature is controlled to within 1° C., the amount of aromatic hydrocarbon can be determined with an accuracy corresponding to 0.10 or less in the percentage by volume. A general procedure is given for determining the aromatic hydrocarbons in a "straight-run" gasoline and in a gasoline containing olefins.

SEPARATION AND RECOVERY OF AROMATIC HYDROCARBONS FROM PARAFFINS AND NAPHTHENES BY ADSORPTION.

The April 1944 number of the *Journal of Research* contains a paper (RP1583) by B. J. Mair and A. F. Forziati of the American Petroleum Institute Research Project 6 describing a method for separating, by the process of adsorption, the aromatic hydrocarbons from their mixture with paraffin and naphthene (cycloparaffin) hydrocarbons, as in the gasoline or kerosene fractions of petroleum. The mixture is introduced into the top of a column containing an appropriate excess of solid adsorbent. A low-boiling paraffin hydrocarbon, such as pentane (or butane or propane), is then added in sufficient quantity to remove from the column the paraffin and naphthene hydrocarbons but not the aromatic hydrocarbons. The latter are then removed by adding an appropriate desorbing liquid, such as methanol. The paraffins, naphthenes, and pentane are thus obtained as a mixture from which the pentane is easily removed by distillation. The aromatic hydrocarbons are obtained as a mixture with pentane and methanol. The methanol is easily removed by extraction with water, and the pentane by distillation.

This method of separation was tested on a known mixture of 17 pure hydrocarbons, the normal boiling points of which covered a range from 60° to 174° C. and included all the 5 normal paraffins from *n*-hexane through *n*-decane, the iso-paraffin 2-methylpentane, the 4 normal alkyl cyclohexanes from cyclohexane through *n*-propylcyclohexane, and all of the 7 possible aromatic hydrocarbons from benzene through isopropylbenzene. The separation of the aromatic hydrocarbons from the paraffins and naphthenes was quantitative within the limits of measurement, and their recovery was complete within the normal operating loss of material in processing.

Experimental determinations were made of the quantity of aromatic hydrocarbon adsorbed per unit quantity of adsorbent, for a number of different binary solutions of aromatic hydrocarbons with paraffins or naphthenes, at several concentrations of the aromatic hydrocarbon, and with silica gel, carbon, magnesia, alumina, Filtrol, and Florisil as adsorbents. The results are displayed in the form of adsorption isotherms.

STANDARD SAMPLES OF HYDROCARBONS.

In *Technical News Bulletin No. 315* (July 1943) mention was made of the preparation at the Bureau of standard samples of hydrocarbons of known high purity for calibrating analytical instruments and apparatus in the research, development, and analytical laboratories of the petroleum, rubber, and allied industries. In order to expedite the work and to make available the largest possible number of compounds in the shortest time, only a limited quantity of each hydrocarbon has been prepared and the purity of each has been pushed only to a point that is believed to be amply adequate for the present urgent needs for calibration. Fifteen hydrocarbons are now available, as follows:

Compound.	Standard Sample Number.	Amount of Impurity.
		Mole per cent.
n-Pentane	201	0.25 ±0.10
2-Methylbutane (isopentane)	202	.13 ±0.06
n-Hexane	203	.24 ±0.09
2-Methylpentane	204	.25 ±0.10
3-Methylpentane	205	(a)
2,2-Dimethylbutane	206	.12 ±0.05
2,3-Dimethylbutane	207	.06 ±0.04
Methylcyclopentane	208	.25 ±0.09
Cyclohexane	209	.012±0.007
Benzene	210	.05 ±0.02
Methylbenzene (toluene)	211	.04 ±0.02
Ethylbenzene	212	.20 ±0.07
1,2-Dimethylbenzene (*o*-xylene)	213	.14 ±0.05
1,3-Dimethylbenzene (*m*-xylene)	214	.17 ±0.07
1,4-Dimethylbenzene (*p*-xylene)	215	.07 ±0.03

(a) Not determined; believed to be the same as for 2-methylpentane.

Each of these hydrocarbons is available at the costs indicated, in Pyrex glass ampoules sealed "in vacuo," in the following kinds and sizes of containers:

Designation to follow Standard Sample Number.	Volume of Hydrocarbon.	Kind of Container.	Cost per Sample.
	ml		
−5	5	Plain ampoule, sealed in vacuo.	$3.00
−8S	8	Special ampoule, with internal vacuum "break-off" tip, sealed in vacuo.	5.00
−25	25	Plain ampoule, sealed in vacuo.	9.00

Proper designation is illustrated as follows: An order for 8 ml. of cyclohexane, sealed in vacuo in a special Pyrex glass ampoule with internal vacuum "break-off" tip, would be given as "Cyclohexane, No. 209-8S." Similarly, an order for 5 ml. of 2,2-dimethylbutane, sealed in vacuo in a plain Pyrex glass ampoule, would be given as "2,2-Dimethylbutane, No. 206-5."

In the interest of simplification, the cost of the various hydrocarbons has been made the same. Prices are subject to change as conditions warrant. The cost includes delivery under government frank in the United States and to Mexico, Canada, Cuba, and United States possessions. For all shipments to other countries, 50 cents postage must be added for each container, and, in addition, 25 cents for insurance or registration of each shipment. Samples must be paid for in advance, with order addressed to the National Bureau of Standards, Washington 25, D. C.

THERMAL EXPANSION OF HIGH-SILICON CAST IRON.

A paper (RP1581) by Peter Hidnert and George Dickson in the April *Journal of Research* gives data on the linear thermal expansion of a high-silicon cast iron containing approximately 14 per cent. silicon with 3 per cent. molybdenum, and of a similar iron without appreciable molybdenum, at various temperatures between 20° and 700° C. Differences between the coefficients of expansion of these high-silicon cast irons were found to be slight. Both were found to have slightly higher coefficients of expansion than electrolytic iron for temperature ranges between 20° and 300° C., and appreciably higher coefficients for higher temperature ranges. No indication of growth similar to that of ordinary cast iron was observed on heating the high-silicon cast iron to 700° C.

COMPRESSIBILITY AND RESILIENCE OF FABRICS.

The compressibility of fabrics has been shown by several investigators to affect the "hand" or "feel" of the materials, particularly their

feeling of softness (*Technical News Bulletin No. 193*, May 1933). Further studies of this property (made at the Bureau by Edwin C. Dreby, during the course of a program on the development of methods for evaluating textile finishes) were sponsored by the American Society for Testing Materials. These point to compressibility as one of several physical characteristics of a fabric affecting to a large extent the judgment of hand.

The studies indicate that the compressibility between pressures of 0.05 and 0.50 lb./in.^{2} affected the judgment of the hand of the soft-finished, lightweight fabrics to which these studies were largely devoted. Therefore, a suitable apparatus for evaluating compressibility must operate at pressures within this range. The apparatus must also give reproducible results for materials having an uneven texture, similar to that of fabrics, with a minimum of time and effort. A compression meter was devised to meet these requirements. By means of the combined effects of compressed air and a column of liquid, pressure is applied to the test specimen through a very thin, pliable membrane that makes possible the application of definite low pressures uniformly distributed over a large test area. The compressibility and compressional resilience are evaluated by observing the decrease in volume of the specimen as the pressure is increased and the increase in volume as the pressure is decreased.

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Studies of the compressional characteristics of a variety of cotton and rayon dress and shirting fabrics, the feel of which had been evaluated tactually by a group of textile experts, showed several ways in which compressibility is appreciated by the hands. The compressional characteristics seem to affect directly the feeling of "thickness." They likewise affect the feeling of "fullness" and it was found that the results of measurements obtained with the compression meter, when combined with measurements of flexibility, gave a quantitative evaluation of the fullness of fabrics. The measurements of fabrics thus far made with the compression meter show it to be at least as sensitive as the hands to differences in compressional characteristics and thus indicate the suitability of the instrument for evaluating the finish of fabrics.

THE CHARLES L. MAYER NATURE OF LIGHT AWARDS.

The National Science Fund announces two Charles L. Mayer Awards for contributions submitted before January 1, 1946 on the nature of light.

A prize of $2,000 will be awarded for an outstanding contribution to our basic understanding of the nature of light and other electromagnetic phenomena which provides in terms intelligible to the community of scientists at large a unified understanding of the two aspects of these phenomena which are at present jointly described by wave and by corpuscular theories.

The second prize of $2,000 will be awarded for an outstanding comprehensive contribution to a logical, consistent theory of the interaction of charged particles with an electromagnetic field including the interaction of particles moving with relative high speeds.

The first award is intended to encourage for the benefit of the nonspecialist the interpretation of facts already known to the specialist. It is hoped that the second award will stimulate attack on one of the most fundamental unsolved problems in physics.

In making these awards the National Science Fund will have the assistance of a special Advisory Committee consisting of Dr. E. U. Condon, Associate Director of the Research Laboratories, Westinghouse Electric & Manufacturing Co., East Pittsburgh, Pennsylvania; Dr. Karl K. Darrow, member of the technical staff of the Bell Telephone Laboratories, and Secretary of the American Physical Society; Dr. Robert A. Millikan, Chairman of the Executive Council, California Institute of Technology; and Dr. I. I. Rabi, Professor of Physics, Columbia University.

Contributions may be submitted to the National Science Fund of the National Academy of Sciences, *2101 Constitution Avenue, Washington, 25, D. C.*

THE FRANKLIN INSTITUTE.

MEDAL DAY MEETING.

WEDNESDAY, APRIL 19, 1944.

Following the innovation introduced last year the annual Medal Day exercises at The Franklin Institute began at 5:30 in the afternoon of Wednesday, April 19th. Members of the Institute who were unable to attend the dinner came to the Institute at 8:00 o'clock for the presentation of medals.

The exercises were begun by a reception to the Medalists followed immediately by dinner. At the conclusion of dinner the programme of the evening was started by the playing of the National Anthem.

Mr. Charles S. Redding, President, presided and, having called the meeting to order, stated that the minutes of the regular monthly meeting of the Institute for the month of February were published in full in the March Journal of the Institute and if there were no corrections the minutes would stand as printed. Upon motion the minutes were declared approved.

The programme and a list of awards follows:

PROGRAMME.

Reception to Medalists ..... THE HOSTESS COMMITTEE

Dinner ..... MEDALISTS, OFFICIALS AND GUESTS

National Anthem

Toast to Benjamin Franklin ..... W. M. VERMILYE
Chairman, National Franklin Committee

Greetings ..... CHARLES S. REDDING
President, The Franklin Institute

Introduction of Medalists of Other Years ..... THE PRESIDENT

"All In It" ..... HENRY BUTLER ALLEN
Secretary and Director, The Franklin Institute

Presentation of Awards ..... THE PRESIDENT

"The Rapidly Expanding Field of Usefulness of the X-Rays" .... WILLIAM DAVID COOLIDGE

"Super-fluidity of Helium II and Attainment of the Absolute Zero" ..... PETER KAPITZA *

"America"

RECIPIENTS OF AWARDS.

Sponsors

Presentation of Certificate of Merit
THE WESTERN UNION TELEGRAPH COMPANY ..... Mr. Edward L. Forstall
Received by A. N. Williams, President

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* Paper read by James Barnes, Ph.D., Technical Adviser, THE FRANKLIN INSTITUTE.

Presentation of Longstreth Medals
FRANK B. ALLEN, President ..... Mr. Frank H. Rogers
The Allen-Sherman-Hoff Company
Philadelphia, Pennsylvania
EDWARD E. SIMMONS, JR. ..... Mr. F. G. Tatnall
Signal Section
Sacramento Air Depot
Sacramento, California
J. STOGDELL STOKES, President ..... Mr. C. H. Masland, 2nd
Stokes and Smith Company
Philadelphia, Pennsylvania

Presentation of Wetherill Medals
RICHARD C. DU PONT, President ..... Mr. Lionel F. Levy
All-American Aviation, Incorporated
Wilmington, Delaware
Awarded posthumously
Received by
Mrs. Richard C. du Pont
WILLEM FREDRIK WESTENDORP ..... Mr. R. H. McClarren
Research Laboratory
General Electric Company
Schenectady, New York

Presentation of Brown Medal
HARVEY CLAYTON RENTSCHLER, Director of Research ..... Mr. C. Sellers, 3rd
Lamp Division
Westinghouse Electric and Manufacturing Company
Bloomfield, New Jersey

Presentation of Clamer Medal
WALTHER EMIL LUDWIG MATHESIUS, President and Director ..... Dr. G. H. Clamer
Geneva Steel Company
Geneva, Utah

Presentation of Henderson Medal
JOSEPH BURROUGHS ENNIS, Senior Vice-President ..... Dr. Rupen Eksergian
American Locomotive Company
New York, New York

Presentation of Levy Medal (in absentia)
STEPHEN P. TIMOSHENKO ..... Mr. Eugene W. Boehne
Department of Theoretical and Applied Mechanics
Stanford University, California

Presentation of Cresson Medal
ROGER ADAMS, Head (on leave) ..... Mr. John M. Weiss
Department of Chemistry
University of Illinois
Urbana, Illinois

Presentation of the Franklin Medal and Certificate of Honorary Membership
WILLIAM DAVID COOLIDGE, Vice-President and Director of Research ..... Dr. Frederic Palmer
General Electric Company
Schenectady, New York

Presentation of the Franklin Medal and Certificate of Honorary Membership
PETER KAPITZA, Director ..... Dr. Frederic Palmer
Institute for Physical Problems
Academy of Sciences
Moscow, Union of Soviet Socialist Republics
Received by The Honorable Andrei Gromyko,
Ambassador from the Union of Soviet Socialist
Republics to the United States

(A detailed statement of these exercises together with the papers presented by the Franklin Medalists, will appear in the June issue of the JOURNAL.)

COMMITTEE ON SCIENCE AND THE ARTS.

(Abstract of Proceedings of Stated Meeting held Wednesday, April 12, 1944.)

HALL OF THE COMMITTEE,
PHILADELPHIA, APRIL 12, 1944.

MR. JOHN B. KLUMPP *in the Chair*, substituting for MR. WILLIAM B. COLEMAN.

Report of Special Committee, Appointed December 8, 1943, to Study the Kilgore Bill (S.702).

An extensive and thorough analysis of the Bill was reported. The following resolution was adopted:

> "The Science and Arts Committee recommends that the Board of Managers of The Franklin Institute ask the Secretary of the Institute to keep in touch with what other scientific and engineering bodies are doing with regard to the Kilgore Bill and that the Institute take such action as may appear advisable to prevent this Bill from becoming law."

JOHN FRAZER,
Secretary to Committee.

LIBRARY NOTES.

The Committee on Library desires to add to the collections any technical works that members would wish to contribute. Contributions will be gratefully acknowledged and placed in the library. Duplicates received will be transferred to other libraries as gifts of the donor.

Photostat prints of any material in the collections can be supplied on request. The average cost for a print 9 × 14 inches is thirty-five cents.

The library and reading room are open on Mondays, Tuesdays, Fridays and Saturdays from nine o'clock A.M. until five o'clock P.M., Wednesdays and Thursdays from two until ten o'clock P.M.

RECENT ADDITIONS.

AERONAUTICS.

WILKINSON, PAUL H. Aircraft Engines of the World. 1944.

ARCHITECTURE AND BUILDING.

American Society of Heating and Ventilating Engineers. Heating, Ventilating, Air Conditioning Guide. Volume 22. 1944.

BIOCHEMISTRY.

BULL, HENRY B. Physical Biochemistry. 1943.

CHEMISTRY AND CHEMICAL TECHNOLOGY.

CHAPIN, WILLIAM H. Second Year College Chemistry. Fifth Edition, Revised. 1943.
CROSS, ROY. From a Chemist's Diary. 1943.
D'ALELIO, GAETANO F. Laboratory Manual of Plastics and Synthetic Resins. 1943.
FRANCIS, CHARLOTTE A., AND EDNA C. MORSE. Fundamentals of Chemistry and Applications. Second Edition. 1943.
HARNED, HERBERT S., AND BENTON B. OWEN. The Physical Chemistry of Electrolytic Solutions. 1943.
HEUSER, EMIL. The Chemistry of Cellulose. 1944.
Institution of the Progress of the Rubber Industry. Annual Report on the Progress of Rubber Technology. Volume 6. 1942.
LEMKIN, WILLIAM. Visualized Chemistry. Fifteenth Edition. 1938.

ELECTRIC ENGINEERING

CAVERLY, DON P. A Primer of Electronics. 1943.
HENNEY, KEITH, Editor-in-Chief. The Radio Engineering Handbook. Third Edition. 1941.

ENGINEERING.

YOUNG, JAMES F. Materials and Processes. 1944.

FIRE PROTECTION

MCCARTHY, JOHN J. The Science of Fighting Fire. 1943.

MANUFACTURES.

CRANE, E. V. Plastic Working of Metals and Non-Metallic Materials in Presses. Third Edition. 1944.
MAHER, JENNIE WISNESKI. Silk Finishing. Revised Edition. 1943.

MATHEMATICS.

RAINVILLE, EARL D. Intermediate Course in Differential Equations. 1943.

METEOROLOGY.

HEWSON, E. WENDELL, AND RICHMOND W. LONGLEY. Meteorology, Theoretical and Applied. 1944.

NAVAL ARCHITECTURE AND NAVIGATION.

HOGG, ROBERT S. Naval Architecture and Ship Construction. 1942.
LAUNER, JAY. The Enemies' Fighting Ships. 1944.
MIXTER, GEORGE W. Primer of Navigation. Second Edition. 1943.

PATENTS.

U. S. Patent Office. Index of Trade Marks 1943. 1944.

PHYSICS.

Physical Society. Reports on Progress in Physics. Volume 9. 1943.

RAILROADS.

Association of American Railroads—Mechanical Division. Car Builders' Cyclopedia. Sixteenth Edition. 1943.

NOTES FROM THE BIOCHEMICAL RESEARCH FOUNDATION.

Studies on the Chemotherapy of Experimental Virus Infections. I. The Effect of Certain Pyrimidine Derivatives on Experimental Murine Poliomyelitis.—DONALD W. MCKINSTRY AND ELIZABETH H. READING. The development of a rational approach to research on the chemotherapy of virus infections is beset with many difficulties. These agents of disease all multiply intracellularly and little is known regarding the mechanism of their pathogenesis. Furthermore, apparently lacking independent enzyme systems, their metabolism appears to be intimately associated with that of the host cell (Hoagland, C. L., Ward, S. M., Smadel, J. E., and Rivers, T. M., *Journal of Experimental Medicine*, **76**: 163, 1942). Indeed their only attribute of life, in the commonly accepted meaning of the term, is the ability to reproduce, and even here the energy and organization of the host cell is necessary. Consequently it is not surprising that they have proved refractory to the antibiotics, sulfonamides and other chemotherapeutic agents which have been so effective in combatting bacterial and protozoan infections (Findlay, G. M., "Recent Advances in Chemotherapy," 2nd Ed., The Blakiston Company, Philadelphia, 1939, pp. 486–93; Kolmer, J. A. and Tuft, Louis, "Clinical Immunology, Biotherapy and Chemotherapy in the Diagnosis, Prevention and Treatment of Disease," W. B. Saunders Company, Philadelphia, 1943, pp. 312–13). It is interesting to note that the more highly parasitic bacteria (Hemophilus and Pasteurella) as well as the Rickettsiae are also resistant to these substances (Kolmer, J. A. and Tuft, Louis, *op. cit.*).

On the basis of our present knowledge regarding these simpler forms of life, one of the most promising methods of attack with drugs appears to be that directed toward the reproductive process. Although the precise mechanism involved here is only imperfectly understood, it seems that these agents, after gaining access to susceptible tissue of the host, exert a directive influence on the synthetic mechanism of the cell diverting it from the normal function to that of synthesizing the virus complex. Since these synthetic processes are believed to be catalyzed by enzymes, it appears probable that virus reproduction may be inhibited either directly by compounds which inactivate these catalysts or indirectly by the phenomenon of drug-enzyme-substrate competition. The latter would involve the use of compounds similar enough in chemical configuration to a virus component to react with the enzyme system concerned but sufficiently unrelated to serve as a normal functional unit of the parasite.

This preliminary communication describes the effect of certain

pyrimidine derivatives on the course of experimental murine poliomyelitis. In view of the fact that many viruses appear to be essentially nucleoprotein macromolecules (Stanley, W. M., *Annual Review of Biochemistry*, 9: 545, 1940), it was hoped that compounds of this nature might possibly inhibit their proliferation by competing with the normal virus pyrimidine components (cytosine, thymine, uracil) for the synthetic enzyme systems involved.

MATERIALS AND METHODS.

Mice. Swiss albino mice, both males and females, weighing 15 to 20 grams each were employed throughout this investigation. They were usually held in the laboratory under standard conditions with a balanced diet several days before infection. Preliminary studies indicated that these animals were uniformly susceptible to the strain of virus used and that the experimental infection ran a true clinical course. All of the mice used were from a single source.

Drugs. All of the pyrimidines investigated were prepared in this laboratory. In addition to those previously described in the chemical literature, several new compounds (4, 8, 9, 13, 19, 20, 21) were synthesized and reported (McKinstry, D. W. and Reading, E. H., JOURNAL OF THE FRANKLIN INSTITUTE, 237: 203, 1943).

Infection. The SK murine strain of poliomyelitis virus * was employed. This strain, which is highly virulent for mice, produces a typical infection characterized by flaccid paralysis of the extremities and rapidly progresses to a fatal end. Maximal virulence was maintained by repeated animal passage. Infected brain tissue, aseptically removed from passage mice sacrificed shortly after the onset of paresis, was used to prepare the inoculum for each test. Several brains were pooled and ground with sterile sand and sufficient 0.1 per cent. glycine † to make a 10 per cent. suspension. After refrigeration overnight at 4° C., gross particulate matter was removed by centrifugation at 1500 r.p.m. for 10 minutes. The experimental infections were produced by intraperitoneal injection of 0.1 ml. of the 10^2 dilution (usually 100 M.L.D.) of a freshly prepared suspension. Titration of the virus preparation employed in each test was conducted simultaneously. The minimum lethal dose was considered to be the smallest amount of virus which killed between 50 and 100 per cent. of the untreated controls during the observation period of 8 days.

Treatment. Graded doses of the compounds suspended in equal parts of distilled water and mucilage tragacanth were administered by gavage. Ten animals with adequate virus controls usually constituted an experimental group. Preliminary toxicity tests and the frank symp-

* We are indebted to Dr. Claus W. Jungeblut, College of Physicians and Surgeons, Columbia University, for the strain of virus employed in this study.

† A small amount of glycine seems to prevent denaturation of the virus.

TABLE I.

Effect of Certain Pyrimidine Derivatives on Experimental Murine Poliomyelitis.

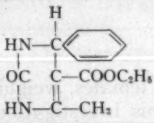
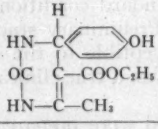
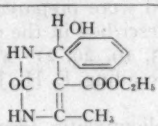
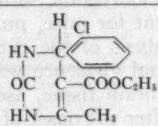
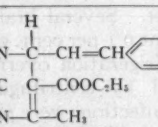
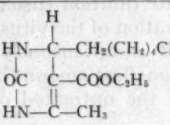
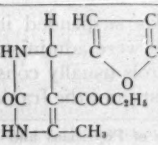
No.	Formula	Dose [1] mg.	Infection M.L.D.	% Survival		Average Days Survived [1]	
				Treated	Controls	Treated	Controls
1		50	100	20	0	5.1	5.8
2		50	100	20	0	5.5	4.8
3		50	100	0	0	4.7	4.0
4		50	100	0	0	4.0	3.0
5		50	100	16	0	4.7	3.0
6		25	100–1000	0	0	2.7	2.0
7		5	100	0	0	1.0	2.0

TABLE I.—*Continued.*

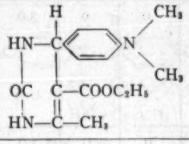
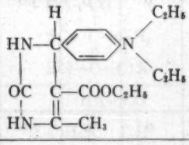
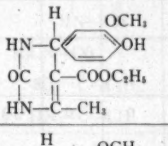
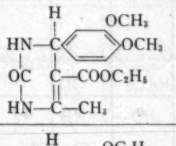
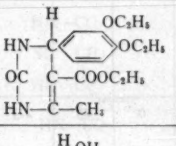
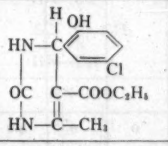
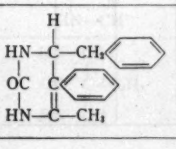
No.	Formula	Dose [1] mg.	Infection M.L.D.	% Survival		Average Days Survived [2]	
				Treated	Controls	Treated	Controls
8		12.5	1–10	0	0	4.0	3.8
9		50	100	0	0	3.3	3.0
10		25	100	11	0	3.9	5.0
11		50	100	0	0	4.7	4.0
12		50	100	0	0	3.8	3.0
13		10	10	0	0	3.5	3.0
14		50	10	0	25	3.4	4.5

TABLE I.—*Continued.*

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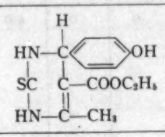
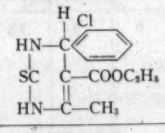
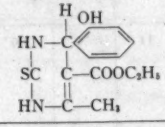
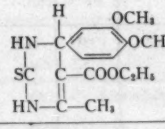
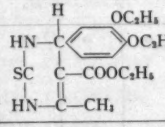
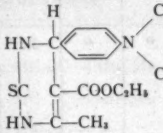
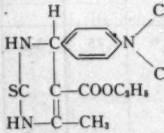
				Treated	Controls	Treated	Controls
15		50	100–1000	0	0	3.0	3.0
16		10	100–1000	0	0	3.0	3.0
17		10	100–1000	0	0	2.0	3.0
18		10	100	0	0	3.3	3.0
19		50	100–1000	0	0	4.7	3.0
20		25	10	0	25	2.3	2.6
21		10	100–1000	0	0	4.0	3.0

TABLE I.—*Continued.*

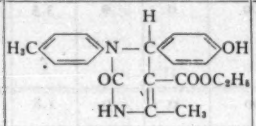
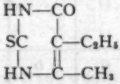
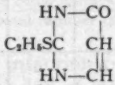
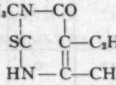
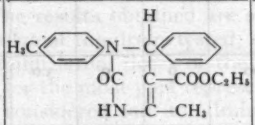
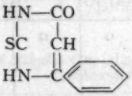
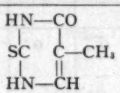
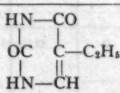
No.	Formula	Dose [1] mg.	Infection M.L.D.	% Survival		Average Days Survived [2]	
				Treated	Controls	Treated	Controls
22		25	10	0	0	3.6	4.0
23		15	100	0	0	3.3	3.5
24		10	100	0	0	4.4	5.0
25		15	1000	0	0	2.0	2.5
26		12.5	10	0	0	3.6	4.0
27		5	10	0	0	3.5	2.0
28		25	100	0	0	3.7	3.0
29		25	10	0	0	3.0	3.0
30		25	10–100	0	0	1.3	3.0

TABLE I.—*Continued.*

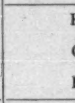
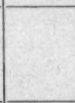
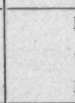
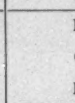
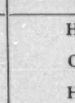
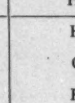
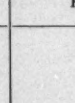
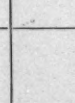
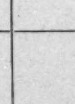
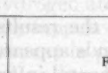
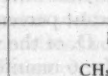
No.	Formula	Dose[1] mg.	Infection M.L.D.	% Survival		Average Days Survived[2]	
				Treated	Controls	Treated	Controls
31		25	1000	0	0	3.5	3.0
32		25	100	0	0	1.8	3.5
33		25	100	0	0	3.0	3.5
34		10	10	0	0	3.5	3.0
35		10	100	17	0	5.5	3.0
36		5	1000	0	0	3.0	3.0
37		2.5	1000	0	0	3.5	2.0
38		25	1000	0	0	2.8	2.0
39		5	1000	0	0	3.0	3.0
40		5	1000	0	0	3.0	3.0

TABLE I.—*Continued.*

No.	Formula	Dose [1] mg.	Infection M.L.D.	% Survival		Average Days Survived [2]	
				Treated	Controls	Treated	Controls
41		5	1000	0	0	3.8	3.0
42		5	1000	0	0	3.5	3.0

[1] Administered twice daily.

[2] $\frac{\text{Number of mice} \times \text{days each survived}}{\text{total number of mice}}$ calculated for observation period of 8 days.

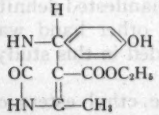
toms of the infection obviated the use of drug controls. Therapy was begun on the day of infection and continued twice daily throughout the observation period. Significant prolongation of life or survival over controls was used as the criterion of therapeutic potency.

RESULTS.

The results obtained are summarized in Table I together with the formulae of the drugs tested, dose in mg. per mouse and the number of minimum lethal doses of the virus administered in each case. These data for the most part represent the therapeutic effect of the maximum dose considered safe to administer on the basis of preliminary chronic toxicity tests. A typical protocol is given in Table II.

TABLE II.

Effect of Ethyl 2-Oxo-4[4-hydroxyphenyl]-6-methyl-1,2,3,4-tetrahydro-5-pyrimidine-carboxylate on Experimental Murine Poliomyelitis.

Drug	Dose [1] mg.	Dilution [2] of virus	No. of Mice	Death of Mice (Days)								Mice Surviving 8 Days	Average Days Survived
				1	2	3	4	5	6	7	8		
	50	10^2	10	0	0	0	0	4	1	3	0	2	5.5
Virus Controls	—	10^2	6	0	0	0	0	1	5	—	—	0	4.8
		10^3	4	0	0	0	0	0	0	2	2	0	6.5
		10^4	6	0	0	0	0	0	0	1	2	3	7.3

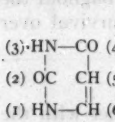
[1] Administered twice daily.

[2] 0.1 ml. intraperitoneally.

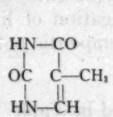
DISCUSSION.

An examination of Table I reveals that although the results obtained were essentially negative, several of the compounds appeared to exert a favorable influence on the course of the experimental infection. Five of the drugs (1, 2, 5, 10 and 35) protected a significant percentage of the experimental animals from infection with 100 M.L.D. of the virus and prolonged the life of the remainder. Compound No. 6 manifested definite therapeutic action against infection with 10 M.L.D. of the virus but was impotent against 100 M.L.D. The other compounds either exerted no effect or only slightly prolonged the life of the treated animals over that of the controls. However, even slight differences here may be significant in view of the fact that treated animals were handled and subjected to the shock of medication twice daily whereas the controls were undisturbed throughout the observation period.

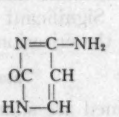
Although the data obtained are insufficient to permit definite conclusions regarding the relationship between chemical configuration and antipoliomyelitis activity with this class of compounds, several observations in this connection are interesting.



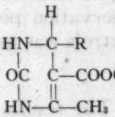
I



II



III



IV

Substitution in the 5 position of uracil (I) or, considered in a different light, replacement of the methyl group in thymine (II) does not appear to endow the compound with antipoliomyelitis activity. Compounds 30, 32, 33 and 34 in which alkyl, halogen, nitro and amino groups respectively were introduced into this position were all without appreciable therapeutic effect. Likewise, replacement of the functional group in position 2 with sulfur or alkylmercapto groups (compounds 24, 28, 29, and 41) and the introduction of methyl groups into positions 3 and 1, 3 (compounds 39 and 40) yielded inactive compounds. However, replacement of the hydrogen atom in positon 6 of uracil with methyl or phenyl groups yielded compounds 35 and 37 which manifested definite antiviral action. The 6-carboxy derivative on the other hand was inactive. The only derivative of cytosine (III) included in this study, isocytosine (compound 42), was relatively ineffective.

The most effective compounds studied were the ethyl esters of certain 2-oxo-4-R-6-methyl-1,2,3,4-tetrahydro-5-pyrimidinecarboxylic acids (IV). Four of the five compounds that protected a significant percentage of the experimental animals from paralysis and death (1, 2, 5, 10) were members of this group. These may be considered as derivatives of uracil (I) in which the functional group in position 4 and

the hydrogen atoms in positions 5 and 6 have been replaced. Antiviral activity appeared to be definitely associated with the nature of the substituent in position 4. The introduction of the phenyl, 4-hydroxyphenyl, n-hexyl, styryl and 3-methoxy-4-hydroxyphenyl groups at this point yielded active compounds whereas a number of other related and unrelated substituents were either inactive or only slightly effective. Furthermore, the introduction of the tolyl group into position 3 (compounds 22 and 26) destroyed the antiviral activity of two of the most effective compounds (1 and 2).

It is hoped that further work, which is now in progress, will reveal compounds of greater therapeutic activity and more exact information regarding the relationship between chemical structure and antiviral properties.

Summary. Five of 42 pyrimidines investigated appeared to exert a slight favorable influence against experimental murine poliomyelitis as indicated by prolongation of life and survival of the treated animals over the controls.

The results are discussed from the standpoint of the relationship between chemical configuration and antiviral properties.

BOOK REVIEWS.

QUANTUM CHEMISTRY, Henry Eyring, John Walter and George E. Kimball. 394 pages, illustrations, 14 × 22 cms. New York: John Wiley & Sons, Inc., London: Chapman & Hall, Ltd., 1944. Price $5.00.

The title of this book indicates a special, if not wider, range of subject matter than is to be found in introduction text books on quantum mechanics. The relationship of this to chemistry is direct when it is realized that if matter is composed of the particles of the atomic nuclei then it must follow that all properties of matter are properties of assemblies of these particles. A knowledge of the forces between these particles and their laws of motion is at least in principle sufficient to determine the behavior of all matter under any conditions. The chemist, with certain additional mathematical tools at hand, can become informed of the quantum theory. The book at hand attempts to put these within reach.

To start with, a brief mention is made of the purposes and extent of the old quantum theory and the reasons for and basis of the new quantum mechanics. The general principles of quantum mechanics are then put forward by making certain postulates from which the whole theory follows leading to conclusions which can be checked experimentally. Differential equations and the quantum mechanics of some simple systems are explained. The use of quantum mechanics is shown in the chapters on the hydrogen atom, approximate methods, and time dependent perturbations. Under the heading of atomic structure are taken up the hypothesis of electron spin and the calculation of energy levels. One of the basic topics of which considerable use is made is Group Theory. This is first given a rather brief and elementary treatment—later actual applications.

A rather interesting section is that where the student is enabled to determine the types of electronic states that may arise in diatomic molecules and how to write orbitals to describe these states. Then it is seen what results can be expected in the simpler cases and there is developed a satisfactory qualitative theory of valence. Following along this path, methods are developed to give approximate quantitative treatment of many electron molecules. Subsequent headings indicative of the coverage are The Principles of Molecular Spectroscopy, Elements of Quantum Statistical Mechanics, The Quantum Mechanical Theory of Reaction Rates, and Electric and Magnetic Phenomena. There are a number of appendixes containing helpful data and a subject index.

Written on the level of the graduate student in chemistry, this book presents a means to become informed of a theory which systematizes all of chemistry.

R. H. OPPERMANN.

PRINCIPLES AND APPLICATIONS OF ELECTROCHEMISTRY (Volume II, Second Edition), by W. A. Koehler. 573 pages, drawings and photographs, 14 × 22 cms. New York, John Wiley & Sons, Inc., London, Chapman & Hall, Ltd., 1944. Price $5.00.

A knowledge of the principles of electrochemistry is required in many technical positions. The applications of electrochemistry are many and a general knowledge of them provides a background for an understanding of important industrial processes. Students as well as those engaged in electro-chemical industries require therefore a carefully laid out course of study, particularly in its early stages. The book at hand is volume two which, together with volume one, comprises a set on principles and applications. Volume one, by Dr. H. Jermain Creighton, covers principles and this one finishes coverage of the subject where a great deal of detail is not required.

The work opens with a discussion of the economics of power supply for electrochemistry; the trend includes primary cells and secondary cells. Different types of electroplating are then taken up including some of the more common plating solutions as well as a few of the solutions

not so widely used. Copper plating solutions are discussed first and the others are brought up alphabetically. Considerable space is given to electrorefining of metals and also to electrometallurgy. Later in the book attention is given to the electrolysis of alkali halides, electrolyte oxidation and reduction, the production of hydrogen and oxygen. The mechanism of corrosion is touched upon as are some of the more common methods of electroanalysis. An interesting coverage is that on arc, resistance and induction furnaces. Others are electronics, the production of ozone atmospheric nitrogen fixation, the separation of two or more materials from each other by electrical means, and a number of other electrochemical processes including the anodic oxidation of metals and electrolytic polishing and etching.

In the back of the book there are author and subject indexes. The book is well illustrated, the progression is logical and well connected, and the style of presentation is simple and direct. Many new topics are taken up in this second edition. This work, with its companion volume one, constitutes a good coverage of fundamentals upon which can be built a more specialized knowledge.

R. H. OPPERMANN.

STATISTICAL ADJUSTMENT OF DATA, by W. Edwards Deming. 261 pages, illustrations, 14 × 21 cms. New York, John Wiley & Sons, Inc., London, Chapman & Hall, Ltd., 1943. Price $3.50.

The purpose of recording data is to provide a basis for action. An adjusted value is a prediction, a derived number based on a partial assimilation of figures. Of course, being an empirical statement it is subject to verification. If done in the proper way from data worth adjusting (good data) good adjustments may result. In social and economic surveys such as in the Census and industrial planning, a method of sampling is used; that is, some characteristics are collected on a sample basis and the tabulations of these sample data need to be adjusted to complete the count. Statistical procedures of this kind are being used to a greater extent in many lines of endeavor.

This book, as the title implies, covers a restricted field of statistical methods. It opens by giving some basic statistical concepts on the meaning of adjustment and some simple illustrations of curve fitting. The method of least squares is explained and the type of problem it is best suited to. For the first time a method of adjusting the observations (finding the calculated points corresponding to the observed points) is provided for the circumstance in which both the x and y coordinates are subject to error. An acquaintance is made with the phenomenon of the instability of equations.

Different kinds of problems of adjustment (e.g., geodesy on the one hand and curve fitting on the other) are here unified and brought under one general principle and one solution. Contributions are given from writers in statistics, least squares and curve fitting.

The methods of this book were developed over a period of sixteen years by the author in government service. They are applicable to many branches of science. The book is a text as well as a reference work and it can be recommended to all those interested in the subject.

R. H. OPPERMANN.

JACOB PERKINS, His Inventions, His Times, & His Contemporaries, by Greville Bathe and Dorothy Bathe. 215 pages, photographs and illustrations, 23 × 29 cms. Philadelphia, The Historical Society of Pennsylvania, 1943. Price $5.00.

Too often is it forgotten that the foundation of our modern technology is a historical introspection of men, science and machines. Aside from furnishing a cultural background and a rounding out of creative desires, a study of the life and work of inventors and contributors to the good of mankind brings forth a knowledge of practical value of abilities and limitations to cope with situations of enterprise and stimulations, of philosophy and psychology.

This biography of Jacob Perkins reveals a great deal about this American inventor not generally known but which provided a lasting influence on present day practice. It is the result of painstaking search over a long period and the skill of the biographer. The story reads in logical order and, as indicated in the title, takes quite some space relating incidents

and descriptions on the economics and general business conditions of the times. Illustrations are especially good. An idea of the diversity of the scientific interests of Jacob Perkins can be had by reviewing the titles of American and British Patents filed by him. These include Manufacturing Nails and Cutting Nails, Bank Note Check Plate to Prevent Counterfeiting, Pumps, Polishing and Graining Morocco Leather, Fire Engines, Water Mills, Manufacturing Spoons, Watermarking Paper, Screw Propeller, Steam Boiler, Steam Engines, Discharging Projectiles by the Force of Steam, Apparatus for Producing Ice and Cooling Liquids, etc. These occurred between the years 1795 and 1838, a period of scientific and industrial growth both here and in England.

In his business and industrial enterprises Jacob Perkins made many friends and a number of partnerships. One of these, it is interesting to note, was Dr. Thomas P. Jones of Philadelphia. Dr. Jones was professor of mechanics at Franklin Institute and editor of the Franklin Journal when Perkins wrote him from London in 1827. The letter was published in the Franklin Journal. It is most astounding in many respects mentioning the steam gun which discharged 500 to 1000 balls per minute, a steam engine working under pressures of 1400 lbs. to the square inch and certain theories on heat. It covers a great deal of ground and many promises, but future events showed that few were ever fulfilled.

The book closes with a number of appendices and a subject index. It is interesting, enlightening and represents a combination of material about the life and work of this great man that is invaluable.

R. H. OPPERMANN.

PUBLICATIONS RECEIVED.

V.2
1944

Testing of Engineering Materials, by Carl W. Muhlenbruch. 200 pages, drawings, illustrations and photographs, 16 × 24 cms. New York, D. Van Nostrand Company, Inc., 1944. Price $2.75.

Physics, A Textbook for Colleges, by Oscar M. Stewart. (Fourth Edition.) 785 pages, illustrations, 15 × 23 cms. Boston, Ginn and Company, 1944. Price $4.00.

The Chemistry of Cellulose, by Emil Heuser. 660 pages, illustrations and tables, 14 × 22 cms. New York, John Wiley & Sons, Inc.; London, Chapman & Hall, Ltd., 1944. Price $7.50.

Mr. Tompkins Explores the Atom, by G. Gamow. 97 pages, illustrations, 17 × 22 cms. New York, The Macmillan Company, 1944. Price $2.00.

Graphical Solutions (Second Edition), by Charles O. Mackey. 152 pages, drawings and illustrations, 14 × 22 cms. New York, John Wiley & Sons, Inc.; London, Chapman & Hall, Limited, 1944. Price $2.00.

Exercises in Second Year Chemistry (Fourth Edition Revised), by William H. Chapin, Werner E. Bromund and L. E. Steiner. 216 pages, drawings and illustrations, 22 × 28 cms. New York, John Wiley and Sons, Inc.; London, Chapman and Hall, Limited, 1944. Price $3.00 Paper.

Introductory Astronomy, by J. B. Sidgwick. 137 pages, illustrations, 16 × 23 cms. New York, Philosophical Library, 1944. Price $2.50.

Handbook of Chemistry, Compiled and Edited by Norbert Adolph Lange. (Fifth Edition, Revised and Enlarged.) 2092 pages, tables and illustrations, 14 × 20 cms. Sandusky, Ohio, 1944. Price $6.00.

Physics, by Erich Hausmann and Edgar P. Slack. United States Naval Academy Edition Revised. 857 pages, illustrations, 14 × 21 cms. New York, D. Van Nostrand Company, Inc., 1944. Price $5.50.